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I. VARIATION IN THE OBLIQUITY OF THE ECLIPTIC.

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THE problem of a variation, or possible variation, in the obliquity of the ecliptic, is one of the greatest interest and importance. As regards astronomy, it is a question which, although it may not materially affect observations made during short intervals, yet will produce most important changes after lengthened periods; and as regards our knowledge of astronomy, it is ten times as important to know whether the obliquity of the ecliptic can and has varied considerably, as it is to decide whether the sun is 90 or 95 million miles from the earth. When we examine this problem from a geological point of view, it is of even greater importance, for it would be the key to the solution of the climatic changes which are known to have existed on earth, and the records of which remain distinctly marked down to the present day.

This problem is one which has not for some years attracted much attention, in spite of its paramount importance; the reasons for this neglect or indifference are probably the following:

First, it has been generally supposed that the question of a variation in the obliquity was fairly and completely examined many years ago; that it was then proved as clearly as that the three angles of a plain triangle = 180°, that the obliquity could vary only 1° 21′; whatever, therefore, were the demands of geology for such an explanation as could be afforded by admitting a considerable variation in the obliquity, still it was supposed that the question had been long since disposed of, and nothing more remained to be done than to admit the impossibility of any variation greater than 1° 21′.

The other reason why this problem has not been re-examined appears to be that there is a belief that it is one which can be judged of only by the most able mathematicians, and can be decided only by aid of the most complicated mathematical investigations; consequently that it was not competent for a mere philosopher or enquirer to offer any opinion on the subject.

With regard to the first of these two reasons, we trust we shall adduce sufficient evidence to show that the *real* question of a variation in the obliquity was not only not settled many years ago, but was not examined, except most partially and most superficially. With regard to the second reason, we believe it can be proved that the problem is one which must be solved long before it assumes the form of a mathematical problem; and, therefore, mathematics have little to do with the main point at issue.

Under these conditions, we purpose in this paper investigating the question of a possible variation in the obliquity, and calling attention to various facts which seem to have been overlooked, neglected, or undervalued, when the problem was examined many years ago.

In the first place, we will bring into notice the evidence in favour of a considerable variation having formerly occurred in the obliquity, and the general evidence in favour of a change being possible; secondly, the evidence or arguments that have been considered sufficient to prove that no greater change could occur than 1° 21′.

The first evidence in favour of a considerable variation in the obliquity is the evidence of geology. Whatever effects may be supposed to have been produced as regards changes in climate by the alternate elevation and depression of land, yet the facts of the glacial epoch demand some more powerful cause than can be attributed to this particular one; that the glacial epoch was universal in both the northern and southern hemisphere, and reached only to latitudes higher than 50°, except in mountainous ranges, are two most important facts, and conclusively demonstrate, we believe, that the elevation or re-distribution of land will not account for such universal results.

Let us for a moment compare the two explanations now before us, and note which appears the more satisfactorily to account for the conditions known to have prevailed during the past.

First, we have the alternations of land and sea, as an explanation of why an arctic climate prevailed down to about 50° latitude in both hemispheres, and produced also glaciers on the mountains in localities within the present tropics. We have this supposed cause to explain these known effects, and we cannot but consider the explanation feeble and inefficient.

Secondly, we have the explanation afforded by the variation of the obliquity to an extent of 12°, thus causing the arctic circle to reach to 54° latitude, and to actually bring this arctic circle down from its present boundary to that very parallel of latitude which the evidence of geology tends to prove was the boundary of glaciers in former times.

Of the two explanations as supposed causes, there is no doubt that the variation in the obliquity alone affords any satisfactory reason for the observed effects, and the rearrangement of land and water can only produce minor changes, which do not touch the vast problem requiring solution.

The question as to a supposed variation to a great extent of the eccentricity of the earth's orbit, is one which has been most ably dealt with by Mr. Belt in the October Number of this Journal. The investigation given by Mr. Belt to the facts of the glacial epoch seem to show that the glaciation of the two hemispheres was contemporaneous, and that there are other effects which are not such as would receive an explanation, even granting that there had occurred formerly a much greater eccentricity in the earth's orbit than that which now occurs.

As an explanation of the known climatic changes which have occurred, we can best test the value of the theory of a considerable variation in the obliquity by endeavouring to explain the observed facts, even of the Boulder period, by this theory, and then by aid of either an increased eccentricity in the orbit or an alteration in the arrangement of land and sea. We believe that it will be admitted that the variation in the obliquity will alone account for the facts in a satisfactory manner, and that other climatic changes, shown by geology to have occurred, will alone be explained by granting a considerable variation in the obliquity. Thus the evidence of geology is so strongly in favour of a variation, both greater and less than at present in the obliquity, that we believe it may be claimed that no other theory has yet been advanced which will account for what is known.

If, as we believe will be the case, the future and present researches of geologists tend to prove that the northern and southern hemispheres were at the *same* time subjected to an arctic climate, it may be fairly claimed that no other theory will so completely and satisfactorily explain the phenomena as a great variation in the obliquity.

We believe also it will be granted that, if it had been long ago admitted that the obliquity of the ecliptic had been 35° instead of 23½°, geologists would at once have accepted this as a full and complete explanation of the great climatic change shown to have occurred on earth, and they would never have searched for those feeble or incomplete causes which are so inefficient when we endeavour to explain facts by their aid.

Thus it may be fairly claimed that geology, the science above all others of facts, is most completely in favour of a great variation in the obliquity, and almost demands it as a cause to explain the known effects.

We will next take the analogy of the planetary system, and we there find that there is no apparently arbitrary law as regards the inclination of the axis of a planet to the plane of its orbit. We find that the axis of Uranus lies almost in the plane of its orbit, so that the arctic circle extends from the poles almost to the Equator. In Venus, nearly the same condition exists, the arctic circle extending to within about 15° of the Equator. In Jupiter, however, the axis of diurnal rotation is nearly at right angles to the plane of the planet's orbit. The inclination of the axis of diurnal rotation of Saturn and Mars does not differ much from 66½°,—the earth's condition.

Analogy in Nature is an argument that must be used with great caution. It does not at all follow that because some planet has an axis of diurnal rotation, so inclined to the plane of its orbit that the arctic circle reaches nearly to the Equator, therefore our own planet was once in the same condition. Whilst, however, we use this caution, we must not forget that, if the analogy of the solar system had been allowed to weigh in the arguments long ago fought out, relative to the form and movement of the earth, its spherical or spheroidal form, and its rotation on its axis, might have been received without much opposition, when it became known that other planets were spheroidal in form, and that they as well as the sun rotated round an axis. The link wanting to bring that which seems probable into the region of proof, is to show that there is now actually going on some movement or some change in the earth's axis which will, may, or might formerly, have caused the angle of inclination of this axis to the plane of the earth's orbit to be a very different angle to that which it now is. This link, therefore, is *the* problem on which rests nearly the whole question connected with the probable or possible variation in the obliquity.

It is a fact known to observers that, during the past 2000 years at least, there has been a change in direction of the earth's axis of rotation, that the rate of this change is about 1° in 179 years; that it is now going on; and that, from the records of the past, this same movement and rate appear to have been nearly, if not quite, uniform.

It is also known from recorded observation that, whilst there has been this change in direction of the earth's axis, there is no evidence of any change of the position of the axis *when referred to the earth itself*. The poles of the earth appear from recorded latitudes to be situated now where they were situated 2000 years ago.

It is also known that the change in the direction of the earth's axis takes place in such a manner that it has produced, during the past 2000 years, little more than half a degree change in the obliquity of the ecliptic.

These recorded facts being before us, we may now advance to the next point in our enquiry, which is to investigate how this problem has hitherto been examined, and in what manner it is still assumed to be disposed of.

As soon as the revival by Copernicus and Galileo of the ancient theory of the earth's rotation on its axis became generally received, it was found necessary to attribute to the earth's axis a movement which should account for the decrease of 20″ per annum in the polar distance of stars, whose right ascension was near 24 hours. At that early date, and up to about 100 years ago, the fact of *any* decrease in the obliquity of the ecliptic was positively denied by the then ruling astronomers. Readers of the "Philosophical Society's Journal," from 1700 to 1750, will find that those persons who believed in any decrease in the obliquity were in the minority, and were treated with but slight respect by the paid official astronomers of the day. It being supposed that no decrease occurred in the obliquity whilst a movement of the pole of the heavens occurred of 20″ annually, it followed that the only possible course of the pole of the heavens to fulfil these assumed conditions was a circular course round the pole of the ecliptic *as a centre*.

The obliquity of the ecliptic being about 23° 28′, it followed that the pole of the heavens traced on the sphere a circle having a radius of 23° 28′ at the rate of 20″ annually. At the period when this supposed movement was agreed to, theory was in the ascendant; the explanation by gravitation of certain celestial movements caused every man who hoped to occupy a prominent position in the scientific world to be a theorist, and to follow and adhere to theories often to the exclusion of facts. The theory proposed by Newton to account for the gyration of the poles of the earth was that the protuberance of the earth about equatorial regions was acted on by the sun and moon, and caused the direction of the earth's axis to vary, though the pole of the earth traced always, it was supposed, a circle round the pole of the ecliptic as a centre.

When the accuracy of modern observation proved that there was a constant decrease in the obliquity of the ecliptic, at the rate of about 0·45″ per century, there seems to have been a great lack of geometrical knowledge among the astronomers occupying official positions, for they still wrote of and described the movement of the pole of the heavens, as tracing a circle round the pole of the ecliptic as a centre, and never varying its distance from this pole, yet admitting that there was a decrease in the obliquity of 0·45 per annum. Now the fact is, that any variation in the obliquity is the same thing as a variation in the angular distance of the pole of the heavens from the pole of the ecliptic, and of the circumference of the circle from its supposed centre, consequently that the pole of the heavens can describe *a circle* round the pole of the ecliptic *as a centre;* whilst we admit a decrease in the obliquity is granting as true that which is impossible. That a grave oversight was here committed it is useless to deny; and though many official astronomers have, since we pointed out* this error, attempted to defend their predecessors, or their own writings on this erroneous problem, yet their endeavours partake more of the attempts of advocates to defend a bad cause than of philosophers desirous of eliciting truth. Personal attacks on an author who has pointed out their errors, and assertions that his problems are by no means believed in by A, B, or C, are not proofs of error, nor can any amount of "authority" outweigh a geometrical proof or law.

When a certain school of teaching have agreed to some problem or theory, have written on this as if it were unquestionable, and have based their calculations on it, it naturally follows that they should at first oppose any innovation on their belief. It does not follow, however, that any number of such men, if they merely make assertions, should carry one atom of weight in disproving that which can be demonstrated by mathematics or geometry. What weight, for example, does Herodotus carry when he asserts that he cannot refrain from laughing when he hears men state that the earth is round, as though made in a machine, and that Asia is as large as Europe. He then asserts the relative size of the two continents, and immediately—like many modern asserters—talks nonsense.

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* In the "Last Glacial Epoch," and "The Proper Motion of Fixed Stars."

It is a law that a decrease in the obliquity of the ecliptic is the same thing as a decrease in the angular distance of the pole of the heavens from the pole of the ecliptic, and is the same thing as a decrease in the supposed radius of the circle described by the pole. Hence—in spite of assertions, of authority, and the many writers who have stated it to be so—it follows that the pole of the heavens does not, and has not at any time within historical periods, traced any part of a circle round the pole of the ecliptic as a centre.

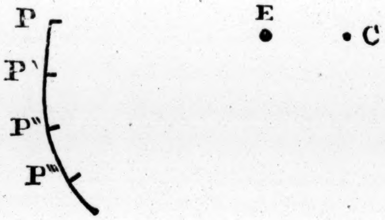
We now come to the most modern explanation of the facts known in connection with the change in direction of the earth's axis, and the decrease in the obliquity. It has recently been stated that the plane (and hence the pole) of the ecliptic is slowly changing its position in the heavens, and thus bringing the pole of the heavens nearer to the pole of the ecliptic; that the pole of the heavens always moves at right angles to the arc by which it would be joined to the pole of the ecliptic; and that the two facts, viz., the movement of the pole of the heavens and the decrease in the obliquity, are thus accounted for.

This is a question either of fact or theory. If it be a fact, it follows that the angular distance of all stars from the pole of the ecliptic—that is, their co-latitudes—will be found to have decreased in one part of the heavens, viz., in that part towards which the pole of the ecliptic is supposed to be moving, and the co-latitude of stars in the opposite part of the heavens will be found to have increased. In order that there should be a decrease in the angular distance of the pole of the ecliptic from the pole of the heavens, it follows that the movement of the pole of the ecliptic should be approximately towards that part of the heavens in which Sirius is situated, and away from that part in which *α* Lyræ is located. To prove this movement, all stars in one direction must have decreased their co-latitudes; all stars in the other direction have increased their co-latitudes. After upwards of seven years' investigation, and after searching every catalogue of stars from that of Ptolemy to the latest, we fail to find *any* evidence of the assertion that such a movement of the pole of the ecliptic has occurred; for there is not any defined change in the latitude of stars to prove that the pole of the ecliptic has moved towards that part of the heavens near the meridian of 6 hours' right ascension.

We have, however, to deal with two well-defined facts, viz., a change in direction of the earth's axis of about

20″ annually, and a closing in—as we may term it—of the pole of the heavens to the pole of the ecliptic of about 0·45″ per annum, at the present time: that is to say, the pole of the heavens traces on the sphere of the heavens an arc, which, when referred to the fixed stars, indicates a rate of about 20″ annually, and towards that part of the heavens indicated by the first point of Aries. This arc approaches the pole of the ecliptic, and may be fairly represented by the arc P P′ P″ P‴, whilst E is the pole of the ecliptic.

FIG. I.



The angular distances, E P, E P′, E P″, E P‴, will be the angular distances of the pole of the heavens from the pole of the ecliptic, at those dates at which the pole of the heavens was located at P, P′, P″, &c.,—and these distances represent the value of the obliquity at those dates. The records of these measures of the obliquity exist, and records of the rate of the movement of the pole exist; consequently the curve P, P′, P″, P‴ can be defined, as regards E, the pole of the ecliptic; and this curve is part of a circle, or part of an ellipse which does not differ much from a circle, the centre of which circle is 6° from the pole of the ecliptic, and so situated that at the date A.D. 2298 the pole of the heavens, the pole of the ecliptic, and the centre C, will be in the same great-circle of the sphere. The curve thus traced out corresponds exactly with the recorded observations of the past as regards the decrease in the obliquity. It explains why the annual decrease in the obliquity is now less than it was found to be one or two hundred years ago, for it will be evident that the nearer we approach the date A.D. 2298 the less will be the rate in the annual decrease in the obliquity, whilst the further we go back the greater will be the annual rate; and this variation in the rate is in strict accordance with recorded observations.

We have, again, another fact to deal with, carrying back into the past the curve (either as a circle or an ellipse very closely corresponding to a circle) thus defined we should obtain a variation of at least 12° in the extension of the obliquity at the date when the pole of the heavens was situated 180° in the circle from the date A.D. 2298. Such an extension of the obliquity would bring England within the arctic circle, and would supply those climatic conditions which geologists consider prevailed during the Glacial Epoch. Thus we have, first, the actual recorded observational facts proving a certain curve to have been traced during the past four or five centuries: secondly, geological evidence, indicating such a climatic change as would result from an extension of 12° or more in the obliquity.

These two evidences are certainly strongly in favour of a possible increase in the obliquity, and, unless very powerful and convincing evidence can be brought in opposition thereto, must certainly tend to prove the fact.

Let us, however, examine another problem. Let us grant that the plane of the ecliptic does slowly vary (of which variation, however, there is no proof from the recorded changes in star latitudes), the course of the earth round the sun would slightly vary; but it is possible that the earth's axis would partake of this movement, and the pole of the heavens and pole of the ecliptic would then vary their relative distances just the same as if the plane of the ecliptic were at rest; and as it is a fact that the course of the pole of the heavens relative to the pole of the ecliptic is such a curve as that we have described it to be, we have actual facts on one side and theoretical conclusions on the other.

Taking an abstract view of the problem as far as it is here stated, we have the following as the two sides of the question relative to a possible variation of the obliquity:—On the one side we have a series of distances measured between the pole of the heavens and the pole of the ecliptic (termed the obliquity at various dates), and also the rate at which the pole of the heavens has decreased its distance from certain stars; consequently the nature of the curve thus traced by the pole of the heavens can be ascertained without any great difficulty. This curve is not one imagined by a theorist, or supposed to be traced in consequence of certain assumptions connected with attraction and repulsion, but the curve is one that recorded facts prove to have occurred during at least 400 years, and this curve—if continued during 13,000 years—will produce a change in the obliquity or angular distance of the two poles, viz., that of the heavens and of the ecliptic of $35\frac{1}{2}°$, and would consequently bring portions of Great Britain within the arctic circle.

That the changes in the latitude of stars in one part of the heavens, and an opposite change in another part of the heavens, indicate that the pole of the ecliptic has moved during past times in any particular direction is not true. This assertion we can bring to the test of recorded facts. The star catalogues of Ptolemy, Tycho Brahe, Hevelius, Halley, and others, can be compared with modern catalogues, and it will be found, by any fair and impartial investigator, that those persons who have so boldly asserted that the proof did exist are more remarkable for their zeal in endeavouring to support a popular theory than they are for that unprejudiced love of truth and fact which should alone be the desire of the man of Science.

The statement that the pole of the heavens always traces an arc at right angles to the arc joining the pole of the heavens with the pole of the ecliptic is a theory. To decide whether the pole of the heavens traces an arc at right angles to the arc joining the pole of the heavens with the pole of the ecliptic, or at right angles to the arc joining the pole of the heavens with the point C 6° from the pole of the ecliptic, is a problem almost impossible to decide by the actual changes in North Polar distance of stars near 24 and 12 hours right ascension. The means, however, by which it could be discovered which course is pursued we have pointed out in detail in our work "The Motion of the Fixed Stars," pages 112 to 170.

The important fact, however, remains,—viz., that if the pole of the heavens trace an arc at right angles to that by which it may be joined to the pole of the ecliptic, then, to account for a decrease in the obliquity, it must be proved that the pole of the ecliptic does move annually towards the pole of the heavens 45″ per century now, and at a more rapid rate formerly, as is shown by recorded observations. Yet no such evidence can be adduced.

Again, suppose it could be shown that the pole of the ecliptic has moved towards that part of the heavens in which are stars having from 5 to 7 hours' right ascension, we still have the stubborn fact before us, that the actual curve traced by the earth's axis is part of a circle having for its centre a point 6° from the pole of the ecliptic, and that if the pole of the ecliptic move towards that part of the heavens occupied for the time being by the pole of the heavens, still we have to account for the curve being such as it is; for to assert that it is a "strange coincidence" that during historical records this curve should coincide with such a circle is little better than a mere subterfuge to avoid admitting a fact which is not popular with a certain party.

Some writers who have opposed the idea of any change in the obliquity greater than 1° 12′ have asserted that it is easy to find a curve that will correspond with the recorded obliquity during three or four hundred years. Such a statement plainly shows that the writers have never attempted to test their assertion; for not only will no other curve agree with the arc thus defined, but if we assume a centre of even 5° 50′, instead of 6°, from the pole of the ecliptic, the curve then traced out will in twenty or thirty years differ several seconds from recorded observations, instead of agreeing with them to within one-tenth of a second for two hundred years. It is unfortunate that writers should make such incorrect statements, and also that persons, in their endeavour to oppose an original investigation, should publish assertions relative to this subject which—had they really read the books* which treat of it—they never could with truth have ventured to write.

As an example of the very small amount of enquiry or thought which has been devoted to this problem, we will call attention to the supposed unanswerable objection which certain writers have brought against the proof of the course traced by the earth's axis being *a circle* round the point 6° from the pole of the ecliptic. These objectors assert that such a course is "impossible," and is against the laws of gravitation, and is opposed to all the analogy of the solar system. They add that *the orbits* of all planets are ellipses, and that therefore no such thing as a circular course can be possible for the pole of the heavens. The repetition, by copying of this statement, shows how powerful it is supposed to be.

In the first place, these objectors forget that the present orthodox belief, which they are endeavouring to defend, is that the pole of the heavens traces *a circle* round the pole of the ecliptic as a centre. When they assert that because the orbit of a planet is an ellipse, therefore the pole of the heavens cannot trace a circle, as such a course is opposed to certain laws, they forget that every zenith traces *an exact circle* round the pole of the heavens every twenty-four hours,—also that the conical movement of a planet's axis is geometrically the same as a second rotation of that planet. Consequently it would be opposed to experience if the earth's axis traced anything but a circle during one revolution of the equinoxes, or, as it may be termed, one second rotation of the earth; and the analogy between the "orbit of a planet" and "the course traced by the pole" is as appropriate as would be the course of a line of rails and the movement of the piston of a locomotive.

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* The Last Glacial Epoch. The Motion of the Fixed Stars.

The problem, however, connected with the possible variation to a great extent in the obliquity of the ecliptic, is one which is considered entirely of a mathematical nature, and the assertion that no variation greater than about 1° 12′ could occur in the obliquity is the result of the investigations of theorists.

When we examine the manner in which this problem has been treated, we find that certain important omissions have occurred, on which the real question at issue is dependent; and instead of the question being one that can off-hand be resolved into a mathematical problem, it is in reality one that can and must be decided long before it assumes the form of a mathematical investigation.

The problem of a possible change in the value of the obliquity to any great amount has been confined almost entirely, hitherto, to an enquiry as to the extent to which the *plane of the ecliptic* could vary from a mean position; and when M. La Place concluded that the plane of the ecliptic could vary only 1° 21′, it was assumed that the obliquity of the ecliptic could vary only that amount. Such a conclusion is incorrect. The obliquity of the ecliptic being the complement of the angle which the earth's axis makes with the plane of the ecliptic, it follows that the direction in which the earth's axis moves is *the* problem for enquiry,—far more important as regards the solution than is that of the variation in the *plane* of the ecliptic.

That the earth's axis was formerly supposed to trace a circle round the pole of the ecliptic, as a centre, is evident from the writings and calculations of all the olden astronomers, and any change in this movement was supposed impossible. It is to this portion of the problem that we will now earnestly call attention.

The present accepted theory of the producing cause of the conical movement or change in direction of the earth's axis is as follows:—The earth is supposed to be a homogeneous spheroid, with a protuberance of about 1-300 at the equatorial diameter. This protuberance,* being acted on by the mass of the moon and sun, causes a gyratory movement of the axis of diurnal rotation, and produces a change in direction of the axis, making it describe a small circle of the sphere round the pole of the ecliptic.

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* See Airey's Lectures.

Let us grant that this theory is perfectly sound, supposing the preliminary data be correct, and we have yet not touched the main question at issue; but let us first examine the preliminary data.

How do we know that the earth is homogeneous?

Let us suppose that every part of the earth's surface in the southern hemisphere is at exactly the same distance from the true centre of the earth,—that the surface in a corresponding latitude in the northern hemisphere is from the true centre,—and it is quite possible that the mass of the two hemispheres is different, and hence the assumption that the earth is homogeneous may be a false one.

Let us, however, come to facts. Let us examine the earth as it is, and deal with what we know. Water finds its own level, and the surface of the ocean in say 50° N. latitude must be at the same distance from the earth's centre as is the surface of the ocean in 50° S. latitude, and so for every degree of latitude north and south. When we examine the globe we find north of the equator the enormous and elevated mass of land comprising Asia, also the whole of Europe, also fully two-thirds of Africa, and the whole of North and part of South America.

These vast continents of land in the northern hemisphere have as a set off, in the southern hemisphere, Australia, a few islands, one-third of Africa, and part of South America. At a rough estimate we may state there is four times as much land, above the sea, north, as there is south of the Equator.

Again, the great depths of the ocean in the Southern Pacific, in the Southern Atlantic, and Indian Ocean, admit of vast quantities of water being now in the southern hemisphere, whereas in the northern there are massive granite mountains, raised 3 and 4 miles above the sea-level, and having in place of water only, 5 miles below the surface, solid granite.

If, then, the materials of the earth be homogeneous, it follows that the preponderance of the land in the northern hemisphere must cause the centre of gravity of the earth to be situated considerably north of the Equator. Every competent mechanician knows that the second rotation of a rotating sphere is, as regards its amount and direction, mainly dependent on the situation of the centre of gravity. If the centre of gravity coincide with the centre of the sphere, regularity of movement may be expected; if the centre of gravity be not coincident with the centre of the sphere, eccentric motion occurs.

In the conclusions arrived at relative to the movement of the earth's axis being such as to produce only one uniform course in the axis, we fail to notice any reference, or any consideration, to the fact that the centre of gravity of the earth does not lie in the plane of the Equator.

But, again, let us examine the globe, and taking half the northern hemisphere—viz., from longitude 15° W. round to about 165° E.—we have the mass of Europe, Asia, and Northern Africa on one side of the sphere, whilst on the opposite side we have only the continent of North America. About three times as much land is above the ocean between 15° W. longitude and 165° E. longitude as there is in the other half, and it therefore follows that the earth's axis cannot pass through the centre of gravity of the earth.

We have, then, at present a spheroid of irregular form, in which the centre of gravity does not lie in the plane of the Equator, nor does the axis of rotation pass through the centre of gravity.

Let us now ask a question. There are forces acting by attraction externally on the earth, and supposed to produce a conical movement of the axis, in consequence of the protuberance at the Equator. Will the movement of the axis of diurnal rotation be exactly the same, no matter whether the centre of gravity of the earth lies in the plane of the Equator, and is passed through by the axis of rotation, or if this centre of gravity is located elsewhere? We need not remind the reader that the movement would not be the same.

Of all the conditions that produce a change in the second rotation of a rotating body—that is, in the change in direction of the main axis of rotation—none is greater than that of a displacement of the centre of gravity, or the fact that the centre of gravity of the rotating body is not coincident with the centre of the sphere. The divergence from a direct course or vertical plane of a spherical shot is always great when the centre of gravity of this shot is not equidistant from all parts of the surface, and this divergence is accompanied by—if not mainly dependent on—a rotation of the shot, in a manner dependent on the position of its centre of gravity.

The period occupied by the earth's axis in tracing a circle round the pole of the ecliptic would be about 25,000 years if the pole of the ecliptic were the centre of this circle, and the period would be about 31,000 years if the centre were the point 6° from the pole of the ecliptic. As the earth rotates about 366 times in one year, it follows that during one revolution of the equinoxes there would be at least 9,150,000 rotations of the earth on its axis. The fact that the centre of gravity of the earth is not situated in the plane of the Equator would produce a different movement of the earth to that which would occur if the centre of gravity were located in this plane; and although during one rotation the differences would be very minute, yet when this difference is multiplied by 9,000,000 the variation would be considerable.

We have, however, other important matters to consider in connection with this problem. We have supposed that the earth is a homogeneous spheroid, but we do not know this, nor can any person know it,—we can only consider it probable; but we can now state what we do know. We do know that in former periods of the earth's history there was a very different distribution of land and sea to that which now prevails, and the results of such changes we can define.

Let us take for granted that the earth is homogeneous, and that at present the centre of gravity is not coincident with the Equator, nor is it passed through by the earth's axis. Now the fact of an elevation of land—say in the southern hemisphere—by the action of internal forces, or the depression of land from any cause, would not make this land weigh more or less, and therefore the relative weight of the land in the two hemispheres would not be altered by any amount of elevation or depression, and no change in the position of the centre of gravity of the earth would be produced by any amount of elevation or depression of the land, *supposing the earth consisted of land only*. Immediately, however, there is an elevation of land, there is a transferral of the water of the ocean over the whole earth. The sea which before covered this land is spread over the whole globe; and when such masses of land as the greater part of Europe and America rose from beneath the sea, millions of tons of water were transferred to other parts of the earth, and the position of the centre of gravity of the earth was consequently altered thereby. The equilibrium (as we may term it) of the earth would not be altered by the actual fact of the elevation or depression of land in either hemisphere, but it would be altered by the resulting transferral of vast masses of water from one hemisphere or one locality, which masses would then be spread over the whole surface of the globe.

The calculations made of the effect produced by the moon and sun on the rotating earth, when the centre of gravity was in one position, could not hold good,—nor would the effects be the same when the centre of gravity was situated in another position. It may appear that the effects resulting from such a cause would be slight, but it must be remembered that whatever effect is produced during one rotation of the earth would have to be multiplied by upwards of 9,000,000 to obtain the effect during one revolution only of the equinoxes; and as 25,000 years is but a small portion of time when compared to the long epochs required by Geology for different periods, it will be seen that this is by no means a fact to be neglected or ignored.

We know, from the researches of geologists, that there was in former times a vastly different distribution of land and sea to that which now prevails. We know that this cause would have produced, by the transfer of the water of the ocean, a change in the position of the centre of gravity of the earth; and, as mechanicians, we know that it is impossible to alter the position of the centre of gravity of a rotating body without altering the movement which the axis of this rotating body may at the time be making. Consequently it follows—if the statement made by geologists be true—that, in former times, continents now above the sea were formerly below that level, and that the change in direction which the earth's axis now exhibits is not that which it formerly pursued; and as the value in the obliquity of the ecliptic is dependent on the course pursued by the earth's axis, it may be affirmed that the variation in the obliquity to a considerable amount is not only possible, but is most probable.

The question of this great change in the obliquity is, we claim, one which cannot be definitely and arbitrarily settled by making certain calculations relative to the attraction of the sun and moon on the protuberant mass at the equatorial regions of the earth, and ignoring all other facts. It is one which no mathematician can deal with correctly, whilst he ignores or overlooks the fact of the present probable position of the earth's centre of gravity, and of the certainty of this centre having formerly been located elsewhere, and also of there having been many changes in its position due to the transferral of the waters of the ocean, in consequence of the many elevations of land in various parts of the earth in former times.

As a summary of this problem we may give the following:—

1st. The course traced by the earth's axis during the past 2000 years corresponds to an arc of a circle, having for its centre the point 6° from the pole of the ecliptic, and so situated that at about the date A.D. 2298 the pole of the heavens, the pole of the ecliptic, and this centre will be in the arc of a great circle. The result of such a course would be to cause an obliquity of about 35° at a date about 13,000 B.C.

2nd. The evidence of Geology is positive as regards the existence of glaciers, in former times, in localities down to about 54° lat. Thus astronomical facts and geological evidence agree as regards this course of the pole.

3rd. The fact that the elevation of land in one hemisphere, or in one locality, will cause a transfer of water, and hence a removal of the locality of the earth's centre of gravity, must produce a change in the movement of the earth's axis, so that this movement cannot be uniform for all time.

4th. The positive assertions of former theorists, that no change greater than about 1° 21′ could occur in the variation of the obliquity is based on two errors:—First, this conclusion was arrived at because it was supposed *the plane of the ecliptic* could vary only to this small amount, and, secondly, because the variation in the position of the earth's centre of gravity was entirely overlooked.

5th. The real problem relative to the variation in the obliquity has little or nothing to do with any change in the position of the plane of the ecliptic, but it has to do with the change in the direction of the earth's axis, and therefore is dependent on the change in position of the earth's centre of gravity produced by the transfer of water from one part to another.

6th. The present somewhat dogmatic theory, that no change whatever greater than 1° 21′ has occurred in the obliquity, assumes that the earth is homogeneous, that the earth's centre of gravity is situated in the plane of the Equator, *and was formerly always where it now is.* That the earth is homogeneous is a speculation only. That the centre of gravity is located in the plane of the Equator cannot be true if the earth is homogeneous. And it is an undeniable fact, due to the known distribution of land and sea in former times, that the earth's centre of gravity was not in the past where it now is.

It thus appears that this problem has yet to be investigated, for the present accepted theory is based on errors or omissions, and is opposed to facts. Its advocates have too often to ignore disagreeable facts, and to start on assumptions which geological knowledge shows us is erroneous. That a curve should now be traced by the earth's axis, which, carried back 13,000 years, will cause an obliquity of 35°, is a fact not to be disposed of by the mere quotation of what the present accepted theories are, especially when geological evidence so positively demonstrates that such climatic changes as would follow this condition actually prevailed on earth. Whilst, however, the earth's axis now traces this course, a different position of the earth's centre of gravity would cause a different movement, and hence the course might have been such in former times as to cause the obliquity to have been far less than it now is,—a condition which would produce almost an uniform climate over the whole earth, as seems to have been the case during the Miocene period.

The present accepted belief, therefore,—that the pole of the heavens always traces a circle round the pole of the ecliptic as a centre, and that the only producing cause of a change in the value of the obliquity must be a change in the position of *the plane* of the ecliptic,—appears erroneous, and to have been based on an imperfect collection of data. No allowance has been made for a change in the position of the earth's centre of gravity, and no knowledge has been shown that the course traced by the earth's axis has even now a definite character, which is not a circle having for its centre the pole of the ecliptic.

The statement, also, that the course traced by the earth's axis in all past times must have been the same that it now is, is the same as affirming that if you alter the position of the centre of gravity of a rotating body you still cause no change in its movements,—an assertion so palpably erroneous that we believe all unprejudiced persons must, when this omission is pointed out to them, admit that their position is untenable, and must grant that a considerable change, both greater and less, in the obliquity is not only possible, but must have occurred.

That vast volumes of water have at various times been transferred from one part to the other of our sphere is a well-known fact. That this transferral must cause a change in the position of the centre of gravity follows as a result; whilst a change in the movement of the axis of rotation also follows as a natural law.

Hence the evidence derived from the course now traced by the earth's axis, from the evidence relative to changes in the earth's centre of gravity inducing a different movement, from the convincing evidence of Geology relative to the former climate of high latitudes, and the absence of any sound arguments or proofs against such changes, lead to the conclusion that the obliquity of the ecliptic not only may have, but has, varied in former times considerably, having been both greater and less than it now is.

That a far less obliquity would give us the uniform climate of the Miocene, &c., whilst a greater obliquity would give us the conditions of the Glacial epoch, is undoubtedly true. It is also undeniable that the course now traced by the earth's axis, if carried back into the past for 13,000 years, would bring about conditions corresponding exactly to those indicated by the geological evidence of the Glacial epoch. That this same curve should be traced in endless gyrations is not, however, probable, and is in reality impossible, when any elevation or depression of the land occurs; and as such changes have occurred, we must trace—from geological evidences of climate—what the former course of the earth's axis probably was.

In conclusion, therefore, we must state that the evidence is considerably in favour of great changes having occurred in the obliquity in past times, that there is no sound mathematical objection to such change, and that the solution of geological paradoxes is probably to be found in this problem. And that the singular opposition offered to its reception by a certain class arises entirely from their not having examined *all* the preliminary data, and from a praiseworthy adherence to their early teaching.

II. THE RIGHTS OF THE THINKER.

A NEW Patent Law is before Parliament, and questions regarding literary copyright have lately been put before the Prime Minister: it is clear, therefore, that some people are interested in defending the men of thought and invention, but it is equally clear from the discussions that neither the lawyers nor the laity have arrived at any clear ideas of the right of property which a man has in his own thoughts. If discovery in the Arts is spoken of, we still have men telling us that they are all the result of accident; scholars have still some floating idea of glass being invented at the casual sight of it formed under the fire made for cooking on some Phœnician river; and Newton's apple accidentally falling seems to be an argument to others. We may throw aside all such fancies; for supposing the glass story to be true, which it almost certainly is not, how many millions of fires had been made in similar circumstances before, without finding an eye to see; and supposing the apple story to be true, as giving a momentary impulse to the thoughtful soul of Newton, how many millions had before seen the selfsame result of falling to the ground, as a characteristic of matter around us. To rear a germ seen in any natural or artificial act up to the fulness of life and usefulness is the work of invention or discovery, as the case may be, and the man who does so creates out of that which was as nothing to the world a property which his fellows value, at times, to an extent which is incalculably great. If a man takes unoccupied territory, producing nothing to his countrymen, and renders it productive, the trifle that he pays for it in any colony is as nothing to that which labour makes it, and no man thinks of denying his rights when he has made a rich estate. He has used the knowledge of Nature, and has made a permanent property for himself and his posterity. If another finds—as in this country—all the land occupied, and seeks, out of other departments of Nature, to bring results valuable to himself and others, he uses frequently much more labour, much rarer knowledge, more varied talents, and with difficulty he obtains a right to his property for a few years only, and then every one seizes it and uses it for himself. Why should a property which a man creates be allowed in his possession for a shorter time than if he finds it? There is one reason, and only one good one, that we know of. If the world of mind had been valued according to its usefulness it would have accumulated to an extent that it would be a burden to the living, and the very progress made would become a clog on its own continuance. This is a reason why the rights should not descend to posterity, but it is not a reason why they should not exist during a considerable part of a man's lifetime, or a certain time after death should he die early. Let a man make an invention every year for thirty years, and let each invention be worth an acre of land,—at the end of the fourteenth year the value of an acre is taken from him, and every year afterwards the same amount is taken away for thirty years, until at last nothing is left. This is the way inventors are treated, whilst peculiar laws are made to retain the property of land in the same family for generations. It is even proposed to take inventors' property away every seven years, as the thriving Jews and other wealthy persons—in times gone past—were robbed for the good of the country or the pleasure of the rulers.

The length of time suited for a patent is worthy of rediscussion. We are showing rather the error of those who would give none whatever. One reason for thus bleeding inventors is, that they take ideas out of a field common to all men. So are the fish of the sea, but he who catches is allowed to keep. Another form of this fallacy is, that some one else would have got the idea; but who knows? The world waits for thousands of years for very simple ideas. There are a few corollaries from other ideas that easily come to many minds, and that is held as a reason why no one should have any monopoly. This is a favourite fallacy: men forget that an idea cannot be made useful to the public without labour and expenditure, and unless a man has some protection he will not risk his money or his time. It is for the interest of the public that there should be a monopoly of a new invention for a time, that it may be worked out and completed, and the market supplied; and if twenty inventors claimed the idea at the same moment it would be a loss to the public if one out of the twenty were not chosen as the owner, or a joint ownership established. In a word, a manufacturer or capitalist refuses to aid in an invention without protection. There are ideas lying dead, because by some patent law they have lost protection. It would be well if the country would renew such patents to men who will undertake them. Why should it be impossible to patent them twice? If the first man in fourteen years could not repay himself, why not give other fourteen years to him, or to another man? And why should a fully-published idea be in all cases a bar to this kind of protection? The knowledge may exist, but the power, or knack, or tact may be wanting, and it is he who benefits the public that the public must reward.

The principle at the root of this reasoning is, that the public gains by giving—for a time at least—the monopoly to individuals. We believe this principle to be fully established.

If we take a country in which patents are not to be procured, we find invention repressed. There can be little doubt that the thoughtful German mind would have done more in industry—a direction extremely natural to it, as its early history shows—had not the laws been destructive of protection to the individual inventor. We do not know if the Government imagines that by refusing patents the country obtains inventions for nothing. The delusion is great; who will show his treasures if they are to be stolen?

The same reasoning applies to ourselves; it is by increasing the protection of the inventor that we shall gain more invention. We know well that many of the best men refuse to carry out ideas because of the expense, which patents will not enable them to recover, because the laws have been a confusion and the administration in the hands of men who do not pretend to a knowledge either of science or the industrial arts.

It has been a principle, on which the patent law is founded, that the good of the commonwealth is the aim. This is fair enough, so far, but we know what injustice was often done under the promising phrase, so powerful in Rome as to make the people blind to the misfortunes it entailed. If the individual must suffer for the commonwealth, it is only because the commonwealth is the protection of the individual, and if it is not he has no interest in suffering for it. We may say, therefore, that the rights of the literary and inventing class are such as the State must protect, as it protects the property in land or potatoes; and if it should not do so it can only be when reduced to a condition of such barbarism as can imagine no property that cannot be measured with a chain or weighed in sacks. In some nations a man might be said to live for the State; in newer times the State is seen, more and more, to be subservient to the individuals, and as new worlds are evolved from the old they must have their position recognised in proportion as they show their value. These new worlds cannot be formed of the same material as this old one, but they are no less admired, and if there is a certain endurance in the matter of this old world which seems destined to give it force as well as respectability, still its highest value is only as a foundation for the growth of the, apparently more transient, gifts of the mind.

Another point connected with patents is not to be neglected. It is said that they must only be given in the name of the inventor. This produces much evil, and causes some men to say that they are inventors when they are not. There is no harm whatever in giving patents in the name of anyone who may say that he is commissioned by the inventor, with whom there may be an agreement. This has been done, and is done, with foreigners, and no good objection has arisen. We have surely a right to this privilege as much as foreigners have. The advantage is, that men may give the benefit of their inventions without being dragged before the world or tormented in law courts, when by constitution they are unable to bear the annoyances. We know the want of such a permission to be a loss to the world of invention. Many who speak in public on this subject seem never to have known inventors or watched the growth of invention. We know that among patentees there are men who may more properly be called thieves, and frighten away some of the best; but as we have seen the growth of not a few inventions, which have been improving year after year, with careful study, labour, and expense, we have learnt, as we imagine any man who thinks at all must learn—in a very simple way—the great value of those of the creative race of Britain who take patents, and we marvel very greatly at legislators who are willing either to throw them aside as too common, or to starve them, in order that they may work hard and produce more. We give the unvarnished meaning of some of the arguments.

We have before us a sketch of a new patent law for Germany. The new patent court is proposed to be partly of lawyers and partly of experts, but there is not room here for a revision of the plan. In Germany the re-issue of patents has been very unsuccessful: the cause may lie very deep—viz., in the constitution of the German mind, which sees principles too much, and not the facts. Facts we know are endless, but if we take the principles out we find them few, and one thing becomes similar to many other things, and in a certain sense the same. The real practical meaning and value of an invention is therefore lost to minds of a certain stamp. In America this has not occurred. It is quite uncertain whether the English mind will be suited to form a court of patents,—it must be tried. To judge from the actions at law we should lose hope; but we may find another result when men are allowed to judge, freed from the trammels of precedent and ancient habit of thought, and free from the irritating arguments so often brought about apparently to complicate the subject. We are inclined to think that no men in Europe could do the work with less bias than the English. We have not studied the American habits on this subject. We may be satisfied, however, that any sensible men could weed out a great many of the applications for patents, and in any disputes come to safer conclusions than can now be obtained by expensive so-called legal processes.

When international law rises somewhat higher out of the region of those inferior quarrels which have so often led men to kill each other, the rights of the inventive and creative mind must take a more prominent place; and the natural tendency is so powerful in this direction that the inability to see it would appear to stamp a man as one of the past, having little sympathy with progress and little tendency to look forward.

We do not see that the time is come for an international patent law,—the interests of nations are not sufficiently alike, even if we include the more advanced nations only,—but in the matter of copyright the union is much nearer the possible, and that nation which advances most in justice and wisdom will assist most readily in forming it. This, however, requires discussion from another point of view, and we shall not continue the subject at present. We do not pretend to have discussed the patent question, but have only brought forward some of the points which seem to have been most neglected, and, being fundamental, are suggestive of many important details.

III. ANOTHER VIEW OF LEVITATION.

IN a recent number of this Journal several instances were given of the ecstatic rising of Saints during prayer or deep absorption of mind. Amongst other things it was said that these levitations, as they were called, did not occur among Saxon Saints. This, we think, is an oversight, as we have the liftings of St. Dunstan—a true Saxon, we suppose, the Dun is clearly not the Celtic noun—and St. Edmund (king), an Angle not easily distinguished from a Saxon. The first will be found narrated, under the name of that Saint, in Butler's "Lives of the Saints;" the second is mentioned in a note under "The Life of St. Philip Neri." It may, however, be true that the Saxon Saints are less connected with the subject than others. This leads to the question, Is it because of the less excitable character of that nation? and we next naturally ask if it is a matter of nerves physically, or is it because of the less imaginative character of that practical race? The tendency of the article connected levitation as among the Saints with modern Spiritualism, which certainly flourishes most in a very excitable, although also practical, nation.

It may be a little foreign to the point to introduce here circumstances, or rather narrations, from a literature almost unknown in England,—levitation of a character more romantic, incredible, and, we may add, fantastical; but, on the other hand, it is a part of scientific enquiry to neglect no view of a question, and if, by going in the direction now taken, we may appear in one instance to be throwing discredit on the whole matter by absurdities, and in another by what some persons might fancy to be profanities, it may, in another point of view, be shown that it is right, in order that we may see to what opinions may lead, or from what remarkable origins they may spring. This is said because it seems uncertain which is the character of the true beginners, although, as a rule, it is the smallest that must be held as such. It is true that we, with most persons, do consider all this Hibernian matter a fine exaggeration, not exceeded by any Americanisms, of which it is perhaps the ancestor; but we give it partly because of the matter being interesting in itself, as belonging to the history of ideas in Physics, and partly to show that the subject is by no means exhausted when viewed from one side only. Scientifically, however, we must not forget that the ideas in the battles of Ireland are like the religious narrations in England and the Continent in exciting times. In the history of the Irish Saints we have an exaggerated levitation, if we may so call it,—but that again runs into the absurd, and it is quite possible that it may have its origin in the minor, and apparently better authenticated, risings during prayer. One of these is about the wildest thing that man has imagined.

The flying of excited heroes is scarcely so well represented in the church, by any example known to us, as by "Christina, a virgin of Tron, who is said to have been carried into the church for burial, when her body ascended from the coffin, and, being recovered from her trance, she related her visions, and ever after was so light that she could outstrip the swiftest dogs in running, and raise herself on to the branches of trees or the tops of buildings."—("Encyclopedia Metropolitana,"—Occult Science.) This is more remarkable even than "Swift Camilla," who skimmed o'er the unbending corn. Savonarolo, as related there, quoting Calmet, is a remarkable instance.

Of course we shall not blame anyone for considering the whole of it like the tale of Mogh Ruith, who called for his white-speckled bird head-piece, with its fluttering wings, and his Druidic instruments, and flew up into the air to the verge of the fires, and turned them northwards against his enemies in a great Druidic battle.—(O'Curry and Sullivan's "Customs of the Ancient Irish," vol. ii.) Or they may be put among such exaggerations as in the fight between Cuchallain and Ferdiaidh, in the same volume, when they made such holes in each other that the birds could fly through them; or when "Cuchallain sprang from the brink of the ford, and alighted at the boss of Ferdiaidh's shield, —upon which Ferdiaidh gave the shield a blow of his left elbow, and cast Cuchallain off from him, as if he were a bird, back to the brink of the ford. Cuchallain again sprang from the brink of the ford, and alighted on Ferdiaidh's shield, for the purpose of striking him on the head." These are stages of the flying, and they can readily be traced through many degrees; and of course we can readily believe that the origin exists in the intensity of wishing in the human soul, by which the slow progress of human beings moving by machinery of flesh and bones envies the additional apparatus of the fowls, and finishes—if it does finish —by balloons. The feeling has its true expression, like many other of our greatest truths, in the words of the Psalmist:—"Oh that I had the wings of a dove, that I might flee away and be at rest."

If the following rather shocks Spiritualists, and sends them to re-consider their facts, it will do no harm, and it will give to those who have not seen any of the wonders of old Irish literature a curious taste, and may add a few readers of its remarkable writings to the small number among us:—In the "Battle of Magh Rath," on Moira, we have an account of the madness of Suibhne, or Sweeny. Taking O'Donovan's translation, as published by the Irish Archæological Society, we find, at p. 231,—"With respect to Sweeny, the son of Colman Cuar, the son of Cobhthach, king of Dal Araidhe, we shall treat of him for another while. Fits of giddiness came over him at *the sight of* the horrors, grimness, and rapidity of the Gaels; at the looks, brilliancy, and irksomeness of the foreigners; at the rebounding furious shouts and bellowings of the various embattled tribes on both sides, rushing against and coming into collision with one another. Huge flickering, horrible, aërial phantoms rose up, so that they were in cursed commingled crowds tormenting him; and in dense, rustling, clamorous, left-turning hordes, without ceasing; and in dismal, aërial, storm-shrieking, hovering, fiend-like hosts, constantly in motion, shrieking and howling as they hovered about them (*i.e.*, about both armies) in every direction, to cow and dismay soft youths, but to invigorate and mightily rouse champions and warriors, so that—from the uproar of the battle —the frantic pranks of the demons, and the clashing of arms, the sound of the heavy blows reverberating on the points of heroic spears and keen edges of swords, and the warlike borders of broad shields, the noble hero Suibhne (Sweeny) was filled and intoxicated with tremor, panic, dismay, fickleness, unsteadiness, fear, flightiness, giddiness, terror, and imbecility, so that there was not a joint or a member of him from foot to head which was not converted into a confused, shaking mass, from the effect of fear and the panic of dismay. His feet trembled, as if incessantly shaken by the force of a stream; his arms and various edged weapons fell from him, the power of the hands being enfeebled and relaxed around him, and rendered incapable of holding them. The inlets of hearing were expanded and quickened by the horrors of lunacy; the vigour of his brain in the cavities of his head was destroyed by the clamour of the conflict; his heart shrunk within him, with the panic of dismay; his speech became faltering, from the giddiness of imbecility; his very soul fluttered with hallucinations, and with many and various phantasms; for that (the soul) was the root and true basis of fear itself. He might be compared on this occasion to a salmon in a weir, or to a bird after being caught in the straight prison of a crib. But the person to whom these horrid phantasms, and dire symptoms of flight and fleeing, presented themselves, had never before been a coward or a lunatic devoid of valour; but he was thus confounded because he had been cursed by St. Ronan, and denounced by the great Saints of Erin, because he had violated their guarantee, and slain an ecclesiastical student of their people over their consecrated trench,—that is, a pure clear-bottomed spring, over which the shrine and communion of the Lord was placed for the nobles and archchieftains of Erin, and for all *the people* in general, before the commencement of the battle.

"With respect to Sweeny, let us treat of him yet another while. When he was seized with this frantic fit he made a supple, very light leap, and where he alighted was on the fine boss of the shield of the hero next him; and he made a second leap, and perched on the vertex of the crest of the helmet of the same hero, who, however, did not feel him, though the chair on which he rested was an uneasy one. Wherefore he came to an imbecile, irrational determination, —namely, to turn his back on mankind, and to herd with deer, run along with the showers, and flee with the birds, and to feast in wildernesses. Accordingly he made a third, active, very light leap, and perched on the top of a sacred tree which grew on the smooth surface of the plain, in which tree the inferior people and the debilitated of the men of Erin were seated, looking on at the battle. These screamed at him from every direction, as they saw him, to press and drive him into the same battle again, and he, in consequence, made three furious bounces to shun the battle, but it happened that—*instead* of avoiding it—he went back into the same field of conflict, through the giddiness and imbecility of his hallucination; but it was not the earth he reached, but alighted on the shoulders of men and the tops of their helmets.

"In this manner the attention and vigilance of all in general were fixed on Suibhne, so that the conversation of the heroes among each other was—'Let not,' said they, 'let not the man with the wonderful gold-embroidered tunic pass from you without capture and revenge.' He had the tunic of the monarch of the grandson of Ainmire upon him on that day, which had been presented by Domhnall to Congal, and by Congal to Suibhne, as he himself testifies in another place.

'It was the saying of everyone
Of the valiant beautiful host,
Permit not to go from you to the dense shrubbery
The man with the goodly tunic.'

His giddiness and hallucination of imbecility became greater in consequence of all having thus recognised him, and he continued in this terrible confusion until a hard, quick, shower of hailstones—an omen of slaughter to the men of Erin—began to fall; and with this shower he passed away, like every bird of prey, as Suibhne said, in another place.

'This was my first run.
Rapid was the flight;
The shot of the javelin expired
For me with the shower.'

And it was by lunacy and imbecility he determined his counsels as long as he lived."

Such is one of the most famous accounts of the Irish on the subject. It is in accordance with a notion there that lunatics lose their weight, at least, to a great extent; very different from the Red Indian who said—"When I am angry I weigh a ton." Can that Member of Parliament have had these stories in his head when he said—"A man cannot be in two places at once, unless he is a bird?" When St. Columba saw the angel flying through the air like a bird, to save a man from falling, the time spent was very short, and the messenger was almost "at once" there, at the end and the beginning.

If we take the ecclesiastical side we hear of many similar flights, such as the instantaneous passage from place to place in Ireland; but probably the Irish fancy could not be brought to tell a simple tale of rising into the air beyond reach: it immediately raised the whole question a few stages higher, and made the immeasurable and incomprehensible out of the strictly limited.

In the notes to the "Life of St. Columba," by Dr. Reeves, as published by Edmondston and Douglas, we have an account of two Saints fighting with demons for the soul of King Brandubh. "Brandubh was killed on the morrow, and demons carried his soul into the air, and Maedhog, abbot of Ferns, heard the wail of his soul as it was undergoing pain, while he was with the reapers, and he went into the air and began to battle with the demons. And they passed over Hy (Iona); and Columbkille heard them whilst he was writing, and he stuck the style into his cloak and went to battle to the aid of Maedhog, in defence of Brandubh's soul. And the battle passed over Rome, and the style fell out of Columbkille's cloak, and dropt in front of Gregory, who took it up in his hand. Columbkille followed the soul of Brandubh to heaven. When he reached it the congregation of heaven were singing *Te decet, &c.* Columbkille did the same as the people of heaven, and they brought Brandubh's soul back to his body again. Columbkille tarried with Gregory, and brought away Gregory's brooch with him,—and it is the hereditary brooch to this day in Iona,—and he left his style with Gregory."

Here we find the utmost exaggeration that one can imagine,—at the same time with limited ideas; but let us go back to Sweeny, whose story, after that of Maedhog and St. Columba's, is like a common occurrence. Let us hear the remark, for a moment, of a modern student of those times. In Note 63 to "Congal," a poem by Dr. Samuel Ferguson, Q.C., we have—"That men should lose their reason in the horrors of hand to hand fighting seems not incredible; but the notion of this kind of phrenetic excitement being attended with a loss of material weight, is, so far as I know, peculiar to Celtic tradition. We have it in the legends of both islands: Merlin, at Arderidd; Sweeny, in this incident of the battle of Moyra; another in the battle of Ventry, and various other instances referred to in the extract following." This is from the "Quarterly Review" of April, 1868:—"The idea, which had a strong and persistent hold of the Gaelic mind, was that excess of mental excitement—especially of the passion of fear—destroyed or counteracted the influence of gravitation. Excessive exaltation of mind arising from religious enthusiasm, whether Christian or Pagan, is alleged to have the same effect.

During the time of those epidemics called Witchcraft, in the Middle Ages, the trial by water was grounded on the assumption that a person demoniacally possessed could not sink; and the test of scales and weights was used, prompted by the same popular conviction. The Irish seem to have confined their belief of the capacity of the human body to receive some influence counteractive of gravitation to the case of lunatics, especially when the phrensy was induced by fear. Thus, in the battle of Moyrath, Sweeny, the young king of Dalaraidh, who has provoked the curse of an angry ecclesiastic, is visited on the battle-field with an excess of terror which deprives him at once of his senses and his bodily weight, and he rises like a leaf or a waif in the air over the heads and helmets of those around him, and so flits rather than—in the figurative sense—flies from the field. The battle of Moyrath is said to have been fought in 637 A.D., and certainly the belief may well be accepted as having been—even at that early period—settled in the popular mind; for here, A.D. 718, in the "Chronicle of the Monks of Clonmacnoise," we find the record of a furious battle between the northern and southern Hy-Niall, at the Hill of Allen, in Kildare, where—together with a great number of kings and chiefs, whose names are given—there perished *novem volatiles, i.e., gealta, i.e.*, nine volatiles or flying phrenetics, who, in the words of the old translator, 'flyed in the ayre as if they were winged fowle.' As usual, the visitation is consequent on the curse of a religious person,—in this case a leper, whose cow had been seized by some of the combatants." Dr. Ferguson continues, "One is naturally led to ask, Can there be such a body of tradition without such a foundation in fact; and, if so, is it a contradiction or negation of the Newtonian law that we are to admit as a solution?"

We may easily answer that, even if we found men floating, Newtonian gravitation would not be interfered with. Balloons, even when rising, still gravitate, and magnets—even rising from the ground by the attraction of another magnet—are not out of the influence of gravitation. It is not at all known whether mental excitement affects weight, but even if it does it can only be to a small extent, and the physical reasoning is quite against it; at the same time, neither the reasoning nor the experiments have been made with such care as men would give had they the slightest hope of gaining any knowledge. The connection of the body with magnetism and electricity is a most obscure subject: there evidently is much to learn; there are movements going on, but in a closed circle, where we cannot observe.

Still, allowing that there was a difference,—say that a man in violent excitement weighed half a pound less,—it would not bring us much nearer these wonderful accounts of flying, and so they must, it is to be feared, go from us like the man in the "Arabian Nights," who sat on a carpet and wished, and was anywhere; or the flying horse of wood, who was moved by the mechanism of a peg; or the older Pegasus himself. St. Columba is the foundation of legends told by men, so rude that the simple abbott was to them as an angel fit for heaven, and able to go when he pleased.

After viewing these ancient narrations we must come to this conclusion,—that there may possibly be something which caused the idea other than mere fancy, but whether that something were merely what we are accustomed to call light-headedness when it is unpleasant, or buoyancy when we feel bright, must, after all, be settled by modern enquiry. We are not wiser till we gain certainty, but we sometimes learn tendencies from indefinite narrations which it is profitable to trace, and it is extremely interesting to think that even the wildest brain has some little germ round which it makes its romance. It is the soul seeking to enlarge its empire of truth, sometimes doing it by empty boasting instead of by conquest. The question remains, What is the conquest in this case, and is there any?

IV. THE HISTORY OF OUR EARTH.*

ONE of the facts which first forced themselves upon the attention of the earlier geologists was the presence, in temperate and arctic regions, of the fossilised remains of animal and vegetable species, such as are now peculiar to tropical or sub-tropical climates. For a time this remarkable phenomenon was referred to the consequences of the Noachian deluge, which was assumed to have brought tree-ferns from tropical forests, and deposited them to form the coal-beds of England and of Nova Scotia. So soon as this crude hypothesis was found to be wholly untenable, it was felt that the climate of the earth must have undergone important secular alterations. For a considerable time it was supposed that during the Cambrian, Silurian, and other pristine periods, the climate of our globe was much hotter than at present, and that it has ever since been gradually declining, as the internal regions of the earth have become progressively colder. Modern physical research, however,* has shown that the earth's general climate could not have been appreciably modified by internal heat for more than 10,000 years after its surface began to solidify, and that the present influence of internal heat upon the temperature does not exceed the 1-75th of a degree. Further, geological evidence goes to prove that—so far from a uniform or progressive decline of temperature having taken place from the earliest ages—there have been periods when the climate was very much colder than at present. There have been, in fact, not one, but several "Glacial periods,"—times when an arctic condition of climate prevailed in the British islands, and when they, along with the greater part of the European continent, were buried under ice. Between these periods, again, there intervened others, when Western Europe enjoyed a semi-tropical climate, whilst even Greenland and Nova Zembla were free from ice, and covered with luxuriant vegetation.

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* Climate and Time in their Geological Relations: a Theory of Secular Changes of the Earth's Climate. By JAMES CROLL (of H.M. Geological Survey of Scotland). London: Daldy, Isbister, and Co.

It must be remembered that a Glacial epoch is not merely a season of unusual cold. Its characteristic feature is the "glaciation" of a considerable portion of the land,—in other words, its becoming coated with a continuous mass of glacier-ice. This phenomenon cannot, obviously, arise from any general decrease of the earth's temperature. Were the sun to be extinguished, evaporation from the surface of the globe would cease, and all downfall, either of rain or snow, would cease likewise. Though, therefore, in such an assumed case the ocean would be frozen down to its very bed, the land would remain bare of ice. For glaciation it is necessary that evaporation should go on unhindered in some warm parts of the globe, and that the moisture thus volatilised should be transferred to colder regions, and there precipitated. This consideration at once disposes of a variety of theories concerning the probable cause of a Glacial epoch, as we shall see hereafter, and proves—what no strictly geological evidence could show—that no such epoch could simultaneously affect both hemispheres.

This premised, we may proceed to examine some of the more important of the numerous hypotheses put forward to account for these alternating epochs of heat and cold.

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* Sir WILLIAM THOMSON, Phil. Mag., January, 1863.

Poisson supposes that the earth may have passed, in its earlier history, through regions of space respectively hotter and colder than the part where she now revolves. On this Mr. Croll remarks, that though there may be a difference in the quantity of force passing through different parts of space in the form of heat, yet we cannot imagine space in itself to be either cold or hot. During the hot periods, therefore, the earth must, on this supposition, have passed near to or among other sources of heat and light in addition to the sun. But masses capable of thus seriously affecting our climate could not do other than greatly perturb the mechanism of the solar system. The orbits of the planets might therefore be expected to give some evidence of such an occurrence.

Others have assumed that the sun is a variable star, its energies waxing and waning at regular or irregular periods. Against this it may be urged—which, indeed, applies also to the hypothesis of Poisson—that a general reduction of temperature over the whole globe could not produce the characteristic phenomena of glaciation.

A variation in the obliquity of the ecliptic is sometimes put forward in explanation of former fluctuations in the earth's temperature. But Mr. Croll declares that "it can be shown from celestial mechanics that the variations in the obliquity of the ecliptic must always have been so small that they could not materially affect the climatic conditions of the globe; and even admitting that the obliquity could vary to an indefinite extent, it can be shown that no increase or decrease, however great, could possibly account for either the Glacial epoch or a warm temperate condition of climate in polar regions."—(Page 8). On this point Mr. Croll is at issue with Mr. Belt, who maintains ("Quarterly Journal of Science," October, 1874) that the Glacial epoch resulted from a great increase in the obliquity of the ecliptic. "There exist," he says, "glacial conditions at present round the poles, due primarily to the obliquity of the ecliptic." "Were the earth's axis perpendicular to the plane of its orbit, spring would reign round the arctic circle," and "under such circumstances the piling up of snow, or even its production at the sea-level, would be impossible, excepting, perhaps, in the immediate neighbourhood of the poles, where the rays of the sun would have but little heating power, from its small altitude." In reply, Mr. Croll remarks that, were the earth's axis to become perpendicular to the plane of its orbit, the quantity of heat received by the polar regions would be far less than it is now. It is well known that at present at the equinoxes, when day and night are equal, snow, and not rain, prevails in the arctic regions, and can we suppose it would be otherwise in the case under consideration? How, we may well ask, could these regions, deprived of their summer, get rid of their snow and ice?" The circumstances which Mr. Belt considers that physical astronomers have left out of account in calculating the variability of the ecliptic, Mr. Croll does not think capable of materially affecting the question at issue. But in the chapter in which he criticises the views of Mr. Belt, Mr. Croll makes certain admissions which very materially qualify his declaration which we have just quoted. He tells us (p. 398) that the "conclusion generally come to by geologists and physicists" is that no great effect can be ascribed to a change in the obliquity of the ecliptic, but that, after giving special attention to the matter, he has been "led to the very opposite conclusion!" "It is quite true," he proceeds, "that the changes in the obliquity of the Equator cannot sensibly affect the climate of temperate regions, but it will produce a slight change in the climate of tropical latitudes, and a *very considerable effect on that of polar regions, especially at the poles themselves*." An increase of obliquity from 23° 28′ to the known maximum 24° 50′ 34″ would cause the poles to receive 1-18th more solar heat than they do at present, and, all other things being equal, would produce "a rise in the mean annual temperature equal to 14° or 15°, raising the temperature of the poles to that now prevailing at latitude 76°." But as the polar regions are covered with snow and ice, this extra heat would have little effect in raising the temperature, and would mainly serve to increase by 1-18th the total yearly amount of ice melted at the poles. Thus the maximum of obliquity, if coinciding with other phenomena to which we shall refer in expounding Mr. Croll's own theory, would have a decided effect in producing the warm interglacial periods.

Let us now examine the theory of Sir Charles Lyell. This great geologist contended that the extraordinary climatic changes of which the "stone book" bears record were due to differences in the distribution of land and water. Were the land all accumulated in high latitudes, and were the intertropical regions occupied exclusively by water, the general temperature of the earth would be lowered sufficiently to account for the glacial period. Conversely, were the land collected within or near the tropics, and were the polar regions occupied by sea, the temperature of the globe would be as strikingly raised. This doctrine

Mr. Croll repudiates. He considers that it "does not duly take into account the prodigious influence exerted on climate by means of the heat conveyed from equatorial to temperate and polar regions by means of ocean currents." Were it not for this heat he maintains that "the thermal condition of the globe would be totally different from what it is at present," and he declares, further, that "the effect of placing all the land along the Equator would be diametrically the opposite of that which Sir Charles supposes." Into this argument it will be necessary for us to enter. The more land is placed at the Equator the more the possibility of conveying the sun's heat from tropical regions by means of ocean-currents is reduced. Heat could, then, only be transferred to the colder regions of the world by means of the upper currents of the trades, since the heat conveyed along the earth's solid crust by conduction is obviously insignificant. But these upper currents, or "anti-trades," are not well adapted for conveying heat. Even at the Equator the upper currents are nowhere below the snow-line, and consequently they play in a region whose mean temperature is below the freezing-point. If warm they would, as a matter of course, raise the snow-line above their own level. The heat carried up by the ascending air-currents at the Equator is not transferred to higher latitudes to be there employed in warming the earth, but is thrown off into the cold regions of space above. The upward current is really one of the most effectual means which the earth possesses of wasting the heat derived from the sun. Consequently, of all places, here ought to be collected the substance best adapted for preventing this dissipation of the earth's heat into space. Now, of all known substances water seems to possess this quality to the greatest extent, and besides—being a fluid—it is adapted, by means of currents, to convey to every region of the earth the heat which it receives from the sun.

Without denying the force of these considerations, it still appears to us that there are certain circumstances which speak in favour of Lyell's supposition. The average temperature of Western and Southern Europe is admittedly higher, latitude for latitude, than that of Asia and North America. But Europe is precisely that portion of the globe which has in its proximity to the south the greatest amount of intertropical land,—to wit, the wide expanse of Africa,—and towards the north the least amount of frigid land. America becomes narrower as we approach the torrid zone, and widens out as we recede towards the pole. Again, hot winds, blowing along the surface of the land, and not depending upon oceanic currents, are not unknown. On the south coast of Australia, a hot north wind, blowing from the heated interior of that continent, is common. In the South of Europe and in South-western Asia warm winds are experienced which cannot be fairly ascribed to the Gulf-stream. On the other hand, the greatest climatic scourge of Western Europe is the north-easterly wind blowing from Siberia and Russia. In America, the "norther"—which sweeps "sword in hand" from the polar regions down to Texas and Mexico—is likewise a land-wind. These facts certainly lend some countenance to the view that the climate of any locality in the temperate zone is lowered by land lying between such place and the Pole, and raised by land lying between such place and the Equator.

Without, however, going further into this question, we can scarcely assume that the land preponderated alternately in the frigid and in the torrid zone for every alternation of hot and glacial periods that the world has experienced. Part of the evidence of a former milder epoch is the discovery of large trees, *in situ*, in Greenland, Spitzbergen, and similar regions. The comparative warmth of the interglacial periods cannot, therefore, be due to the absence of land in high latitudes.

It has been ingeniously suggested that a warm epoch might be produced by an alteration in the composition of the atmosphere. Carbonic acid gas, whilst perfectly pervious to heat-rays emanating from a body of high temperature, such as the sun, is almost impassable for heat-rays of low tension given off by non-luminous bodies. Did our atmosphere contain a few per cents of this gas, the earth would be warmed by the sun's rays, during the day, precisely as at present; but at night the loss of heat by radiation would be practically annulled, and the earth consequently would lose little or nothing of the heat absorbed during the day. But though this theory might account for a single warm epoch, it cannot explain a succession of intensely cold periods, separated by a series of warm intervals.

We must now consider Mr. Croll's views on the cause of the Glacial epochs,—a theory which has a certain resemblance to that of Adhémar, if we strip the latter of certain of its exaggerations.

There are two circumstances which influence the relative position of the sun and the earth, and which, to a very great extent, affect the climate of the latter. These are the precession of the equinoxes and the periodical changes in the eccentricity of the earth's orbit. In the northern hemisphere, at present, winter occurs when the earth is nearest to the sun. As at that period she travels in her orbit with the greatest speed, it results that our winter half-year—*i.e.*, the interval between the autumnal and the vernal equinox—is nearly eight days shorter than the summer half-year. In the southern hemisphere the conditions are, of course, exactly reversed. In virtue of the precession of the equinoxes, however, times have been—and will be again—when the northern hemisphere will have its winter when the earth is in aphelion, and its summer when she is in perihelion, and when it, in turn, will have a longer winter and a shorter summer. But the orbit of the earth is also undergoing a regular series of changes. Its "eccentricity is at present diminishing, and will continue to do so during 23,980 years from the year 1800 A.D." After this time it will again gradually increase. The earth's distance from the sun, when in perihelion, is at present 89,000,000 miles. But when the eccentricity of her orbit is at its superior limit it will have fallen to 84,000,000, whilst her distance when in aphelion will be no less than 98,000,000. Hence, when the eccentricity is greatest, that hemisphere which has its winter solstice in aphelion will have its winter longer than its summer, not by eight, but by thirty-six days. The direct heat of the sun would be one-fifth less during that season than it is at present. Hence there would be greater facilities for the accumulation of snow and ice, and less opportunity for their disappearance during the summer. Now there is perhaps no effect which reacts upon and intensifies its cause so decidedly as do snow and ice. These substances, when once accumulated, lower the temperature in various ways. No matter what the intensity of the sun's rays may be, the temperature of ice and snow can never rise above 32° F. "In Greenland," as Mr. Croll reminds us, "a country covered with snow and ice, the pitch has been seen to melt on the side of a ship exposed to the direct rays of the sun, while at the same time the surrounding air was far below the freezing-point." The atmosphere is chilled by contact with the snow-covered ground, and is not warmed by the radiation from the sun. Again, the greater part of the rays which fall on snow and ice are reflected back into space and wasted. Those which are not reflected away cannot raise the temperature, since they are consumed in the mechanical work of melting the ice. Further, snow and ice chill the air, and condense its vapours in the form of thick fogs, and thus effectually screen themselves against the rays of the sun.

Now we are well aware that at present the southern hemisphere is colder than the northern. "Sandwich Land, which is in the same latitude as the North of Scotland, is covered with ice and snow the entire summer; and in the island of South Georgia, which is in the same parallel as the centre of England, the perpetual snow descends to the very sea-beach. In the Straits of Magellan, lat. 53° S., the direct heat of the sun ought to be as great as that in the centre of England. Yet in the midst of summer, when the day was eighteen hours in length, the thermometer seldom rose above 42° to 44°, and never above 51° F." Icebergs 1000 feet in height have been sighted in latitudes as low as 37° S.

If we now imagine that the earth's orbit were at present about or near its limit of maximum eccentricity, the southern hemisphere, having its winter solstice in aphelion, would have a summer too short, not by eight, but by thirty-six days, and would now be suffering all the rigours of a Glacial epoch. Great part of Australia, New Zealand, South Africa, and of temperate South America, would be overlaid with "thick-ribbed" ice. The region of highest temperature, instead of lying, as now, near the Equator, would be driven northwards as far, perhaps, as the tropic of Cancer, whilst the southern half of the torrid zone would be barely habitable. Meantime the condition of the northern hemisphere would be just the reverse. Its winter half year would be thirty-six days shorter than the summer. Thus the portion of the year during which snow and ice could accumulate would be largely abridged. Every season would see a decrease in the circumpolar ice-mass, and a corresponding quantity of the sun's rays, no longer wasted in thawing snow, would be available for raising the general temperature of the arctic regions.

A natural result would be that trees such as now flourish in Central Europe, would grow in Greenland and Spitzbergen —as has been the case aforetime—whilst Western Europe would display a vegetation of a semi-tropical character. A great period of this kind, when a high eccentricity of the earth's orbit will produce Glacial epochs alternately in the northern and southern hemispheres, awaits us in the remote future. "It consists of three maxima, one hundred thousand years apart. These will occur at the periods 800,000, 900,000, and 1,000,000 years to come."

It is interesting to examine how far this view of a Glacial epoch, successively invading either hemisphere, harmonises with certain suggestive speculations put forward by Mr.

Belt in his delightful "Naturalist in Nicaragua." * This eminent investigator urges that in a Glacial epoch a vast proportion of water must have been piled up upon the continents in the shape of ice and snow; that the level of the ocean must have been thus greatly lowered, and that wide territories, now submerged, must have been the homes of man, and of organic life in general. By this assumption he explains certain points in the migrations and distribution of plants and animals, and throws a light upon the deluges of the ancient world, and upon the myth of Atlantis.

According to Mr. Croll, a vast amount of water would, indeed, be accumulated in the solid form in the glaciated hemisphere. He considers that even now the "antarctic ice-cap" extends from the pole down to lat. 70° S., being 2800 miles in diameter. The whole of this region, he concludes, is covered "with a continuous sheet of ice gradually thickening inwards from its edges to its centre. A slope of one degree, continued for 1400 miles, will give *twenty-four miles* as the thickness of the ice at the pole. But suppose the slope of the upper surface of the cap to be only one-half this amount, viz., a half degree—and we have no evidence that a slope so small would be sufficient to discharge the ice—still we have twelve miles as the thickness of the cap at the pole." As there are in the southern ocean icebergs above a mile in thickness, the great antarctic ice-cap must be in some places over a mile in thickness at its very edge! Were one mile of ice melted off the whole extent of this southern cap, it would raise the general level of the ocean 200 feet!

If the ice-mass at the South Pole is at present so enormous, what must the amount have been surrounding the pole of a glaciated hemisphere? Mr. Croll does not, however, consider that the total amount of ice upon the earth's surface during a Glacial epoch could be greatly in excess of what it is at present. He holds that what is now, with some approach to equality, distributed between both poles, was then accumulated around one. The effect of this upon the general level of the ocean would depend not so much upon the direct decrease of water owing to its being converted into ice, as to an indirect action. Remove all the ice from one pole and transfer it to the other, and you affect the centre of gravity of the globe. Two miles of ice withdrawn from the southern circumpolar continent would displace the earth's centre of gravity by

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* QUARTERLY JOURNAL OF SCIENCE, vol. iv., p. 320.

190 feet. If one-half of this mass were further added to that existing at the North Pole, an additional displacement of 95 feet would ensue, making a total of 285 feet. The immediate result of this would be a rise of the sea level in the northern hemisphere—which in the latitude of Edinburgh would amount to 234 feet—and a corresponding fall of the sea level and laying bare of lands now submerged in the southern hemisphere. The removal of 12 miles of ice from the South Pole would effect in the latitude of Edinburgh a rise of the sea to the height of 800 to 1000 feet above its present level. This is on the supposition that the interior of the earth is solid to the centre. If it be fluid, the displacement of the centre of gravity and the consequent modification of the sea level would be much greater.

Both Mr. Croll and Mr. Belt, then, agree that the occurrence of a Glacial epoch must greatly affect the distribution of land and water, and might easily produce cataclysms,—in case, for instance, that ice-masses melted away very rapidly, and especially if they disappeared more quickly in one locality than they formed in another. Mr. Croll—though he does not enter at length into this matter—admits that the withdrawal of the water from shallow seas would connect lands now isolated, and allow of the migration of plants and animals from one continent to another, or from continents to what are now islands. The depth of Behring's Straits does not exceed 30 fathoms. A lowering of the ocean level in the northern hemisphere to the extent of 200 feet would therefore connect the Old and New Worlds, and allow of an interchange both between their plants and their animals. Such a lowering could only occur when the southern hemisphere was glaciated, and the northern was enjoying its warm interglacial period. Consequently organic forms, now only found in the temperate regions of Asia, would press on into Siberia, cross the bridge, and establish themselves in America, where, when the temperature became less genial, they would gradually migrate southwards. Similarly in Europe, during the interglacial period, England would be part and parcel of the Continent, and the beds of the North Sea and the Channel would be dry land. Hence animals of southern regions were enabled to enter Britain, and have left their fossilised bones in proof of their former presence. The existence of closely allied or identical forms in countries now separated by wide seas, has led some naturalists to the assumption of a double origin, whether by special creation or by development—a view for which there is now no necessity. It is a remarkable fact, easily understood on the supposition of such changes of sea level, that the animal population of two neighbouring countries separated by the sea is more evidently unlike in proportion as such sea is deeper. No probable alteration of sea level is likely to have established a bridge between Africa and Madagascar. Consequently we discern in their mammals, and even in their insects, the most remarkable discrepancy. Not one of the monkeys, the cats, or the antelopes, in which Africa is so rich, has been found in its island neighbour. Both territories are rich in Cetonias, but the species of South-Eastern Africa are not the same as those found on the opposite shore of the Channel of Mozambique.

One of the most interesting facts in animal geography is the existence, in southern temperate regions, of forms which belong to the northern temperate, or even the subarctic zone. Conversely a few species from extreme southern regions have reached northern temperate climates. The question has therefore been asked by Mr. Belt and others* how the plants and animals of temperate or arctic climates could cross the Equator in their migrations from one hemisphere to the other? The alternate occurrence of a glacial period in either hemisphere solves the problem, as was, in fact, pointed out by Mr. Darwin. Suppose a cold period prevailing in the northern hemisphere and a warm one in the southern, both at their maximum intensity. The thermal equator, instead of, as at present, approximately coinciding with the geographical equator, would be driven southwards to—or perhaps beyond—the tropic of Capricorn, whilst the northern half of the torrid zone would experience a temperate, or perhaps even arctic climate. That such a state of things has actually prevailed in inter-tropical regions, and that indubitable marks of glaciation exist at comparatively low levels in Central America, has been fully shown by Mr. Belt. The animals and plants of northern extra-tropical regions would then easily manage to reach the geographical equator. When, again, the glacial period of the northern hemisphere began to relax and the thermal Equator returned gradually towards its normal position, these organic forms would take refuge on the mountains, thence to re-descend and continue their southward journey, when the region of greatest heat had passed to the northwards. For a more detailed exposition of this process the reader may consult Darwin's "Origin of Species" (sixth edition, p. 339).

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* Quarterly Journal of Science, Oct., 1874.

The differences of view between Mr. Belt and Mr. Croll may be summed up as follows:—The former ascribes the former emergence of regions now under water to the general decrease of the amount of water present in a fluid state, while the latter attributes the same phenomenon to the accumulation of water in the glaciated hemisphere. Mr. Belt considers that low-lying lands might be left bare in the immediate vicinity of glaciated regions—*e.g.* territories now covered by the Caribbean Sea during the glaciation of Central America—and might serve as a refuge for tropical forms of organic life. Mr. Croll, on the contrary, holds that the sea can only recede in the non-glaciated hemisphere, and must of necessity rise in the glaciated above its present level. Finally, as is implied in the foregoing remarks, Mr. Belt assumes—if we do not misunderstand him—that a Glacial epoch might simultaneously invade both hemispheres, whilst if Mr. Croll's hypothesis is well-founded, it is, of necessity, limited to one.

Both these authors consider—and we must admit that in so doing they are warranted by facts—that of the evident alternations of land and sea noticed even by Ovid, many have been caused by a local increase or decrease of the depth of the latter. But neither of them asserts that all such changes are to be ascribed to this cause, or denies the existence of those areas of elevation and of subsidence to which Mr. Darwin draws attention in his "Naturalist's Voyage." The question is one which can be settled by careful observation. If we find that the sea gains upon the land simultaneously throughout the whole of one hemisphere, most markedly in high latitudes, and to a less and less extent as we approach the Equator, we should then be warranted in concluding that the encroachment was due to an increasing accumulation of water in that hemisphere. But if we observe the land in South America gradually rising higher and higher out of the water, whilst in certain of the South Sea islands it is gradually sinking, we should then be justified in seeking the cause in the crust of the earth, and in pronouncing South America an area of elevation and the Pacific Isles an area of subsidence.

Incidentally we have referred to the theory of M. Adhémar. This author seems to have been the first who pointed out the possible displacement of the earth's centre of gravity by means of an ice-cap accumulating on either pole. But he supposes this cap to rest, not upon circumpolar land, but upon the bottom of the sea, and to attain the height not of 12 or 24 miles, as Mr. Croll supposes, but of

60! Each hemisphere, he assumes, is in turn colder and warmer. The sea retires on the warm hemisphere, where the ice-cap is decreasing, and gains ground on the colder hemisphere, where ice is accumulating. In a period of 10,000 years the conditions are reversed, the reversal being not incidentally, but inevitably attended with a universal deluge. The last catastrophe of this kind was the Noachian flood, when the great ice-cap then existing on the North Pole collapsed, and the preponderating amount of water previously collected in the northern hemisphere rushed across the globe to its present position in the south. On this hypothesis the southern ice-cap will continue to increase for about another thousand years from the present date, whilst the southern hemisphere becomes still colder, and the sea gains ground upon the land. At the expiration of that time the process is reversed; the southern ice-cap begins to decrease, and its northern antagonist to preponderate. The maximum amount of water recedes from the southern hemisphere and encroaches upon the land in the north. Finally, when 10,000 years from the days of Noah have expired, and when the winter of the southern hemisphere approaches perihelion, "the ice grows soft and rotten from the accumulated heat, the sea begins to eat into the base of the cap, which is so undermined as to be left standing upon a kind of gigantic pedestal. This disintegrating process goes on till the fatal moment at length arrives, when the whole mass tumbles down into the sea in huge fragments, which become floating icebergs. The attraction of the opposite ice-cap, which has by this time nearly reached its maximum thickness, becomes now predominant. The earth's centre of gravity suddenly crosses the plain of the Equator, dragging the ocean with it, and carrying death and destruction to everything on the surface of the globe."

We need scarcely say that this theory harmonises ill with positively established facts in physics, climatology, geology, and animal geography. We have no evidence that all land animals and plants have been periodically eradicated every 10,000 years. We have proof—almost, if not quite, absolute—that certain parts of the earth's surface at no extraordinary altitude, have not been submerged for a length of time greater than this theory would imply. We have good evidence that the polar caps rest, not upon the bottom of the sea, but upon land, and we consider that in the former case the probability of their melting away and breaking up would be but slender. In short, this theory may be held up as a warning to mathematicians who persist in rearing up imposing superstructures upon imaginary data. That the respective distribution of land and sea in the two hemispheres may have fluctuated greatly from changes in the earth's centre of gravity, and that partial and even extensive deluges may have occurred, is a perfectly reasonable supposition.

V. DIFFICULTIES OF "DARWINISM."

MUCH of the unprofitable ink-shed by which the so-called "Darwinian controversy" has been obscured has sprung from the neglect of a very plain distinction. Evolution — the gradual mutation of organic forms in accordance with definite laws, and the consequent origin of what we call "species"—may be a fact, as the writer is by no means disposed to question. But its truth does not necessarily involve the truth of the doctrine commonly known as "Natural Selection." It may yet be shown that this theory, supplemented by Sexual Selection, will account for all the unsolved mysteries of the organic world. Or it may—and probably will—be found that we require the aid of some law of a higher order, defining the limits and the conditions under which the great task of Evolution is carried out. Or, lastly, it is by no means impossible that the entire theory of Natural Selection may have to be thrown aside. Its future depends simply on its power of explaining facts. So long and so far as it is able to do this, it is our duty to follow it. But if we find it fail us, we must be prepared either to supplement it with some more efficient conception or to abandon it altogether. It is with a view of testing this theory that the following *nuces zoologicæ* are submitted to the world. The writer does not hold that they can be more fully solved on the hypothesis of distinct and special creation than that of Evolution. But the reverse ought to hold good. Unless a new doctrine does us better service than the old one, how is it to be legitimated ?

Let us first examine the colouration of animals, and consider in how far it can be accounted for by Natural Selection supplemented by Sexual Selection. In the Mammalia the first point which strikes us is the very limited range of colour which they possess. We question if a single instance of a true prismatic shade can be shown in the whole class. The reds found in certain ruminants—*e.g.*, in domestic cattle—are merely reddish-browns. The yellows and oranges of the cats, the fox, and the jackal, are simply buffs or *cuirs* —"cures" as the English dyers call them. Oken, in his great work, concluded a chapter on the Races of Mankind by asking why there were no green or blue men? His question was received with a somewhat gratuitous amount of ridicule,—certainly the easiest, if not the most satisfactory, way of disposing of it. But it may surely be asked why there are no green or blue mammalian species?* In beasts, again, iridescent colours—so common in other departments of the animal creation—are wanting, save in the golden mole of Siberia. We find no other case where the fur of a beast displays different colours according to the position from which it is viewed. Nor is there any near approach to the metallic brilliance so commonly met with amongst birds, reptiles, and insects. The prevailing garb of beasts may, in short, be characterised as monotonous, flat, and sombre in its tones; nor is the pattern or design in which the colours are arranged much more striking than the shades themselves. The majority of species are either concolourous or irregularly blotched and clouded. Even the zebra, the striped and spotted cats, and the spotted deer display nothing which can be brought into comparison with the elaborate and delicate designs found in birds and insects. Whence, then, comes the difference? Not, surely, from any distinction in the diet of the two classes. There is no article of food used by birds which is not also eaten by some beasts. The hair, fur, and bristles of beasts cannot be said to differ chemically from the feathers of birds, so that we can see no reason why the one material should not display the same colours as the other. The old school of Natural History would cut, rather than untie, the knot, by referring all to the "*sic volo, sic jubeo*" of the Creator; but, without in the least questioning the existence of a superintending Intelligence, we cannot regard such an explanation as scientific: it amounts to little more than the well-known domestic argument —"It is because it is." What, then, can Evolutionism—as at present understood—do towards solving the difficulty? Certain points connected with the colouration of animals it has explained with no small felicity; it has shown that the uniform tawny hue of the lion, resembling the soil of

* Meaning, of course, species with blue or green fur, hair, or bristles. The blueness of the nose in certain baboons can scarcely be taken into account here.

the deserts which he inhabits, screens him from the observation of his intended prey; it has pointed out that the tiger, lurking amidst the perpendicular stems of reeds, flags, and high grasses, is concealed by his vertical stripes. On the other hand, the rosettes and ocellated spots of the leopard, and other truly arboreal cats, are equally well adapted for hiding them amidst the foliage of trees. But surely the concealment of these latter species would have been still more complete had their ground-colour been some shade of green instead of buff, or stone-colour, as we actually find it. If Natural Selection gave them their rosettes, why did it not act further in the same direction, and assimilate them to their haunts, not merely in design, but in colour? There are, also, several species of beasts, frequenting grassy plains, which would have been much less conspicuous, and would have more readily escaped the observation of their enemies, had their colour approximated to that of the surfaces around them.

The zebra is, in colouration and design, very similar to the tiger,—a resemblance which Swainson accounts for by declaring them both eminently sub-typical forms of their respective groups, as shown by their fierce and untameable dispositions. That the stripes of the tiger favour it in escaping the notice of its destined prey, and that they are therefore explicable on the principle of Natural Selection, is admitted. But is the zebra, when grazing, screened in the same manner from the observation of an approaching enemy? He frequents pastures similar in their general character to those sought by the concolourous horse and ass, and by the very imperfectly striped quagga. When feeding, further, he stands so much higher than a crouching tiger that he would be hidden only among very lofty reeds or grasses. Moreover, it may be remembered that a troop of zebras, when pasturing, generally have a sentinel placed in some suitable position, who warns his comrades if he observes anything of a suspicious nature. He has, therefore, less need of a colouration calculated to mislead an enemy.

The pattern of the giraffe somewhat resembles that of the leopard, but it is very questionable in how far it can promote his safety. He browses, indeed, upon the lower branches of trees in the outskirts of woods; but this is surely a very different position from that of the arboreal cats crouched aloft among the dense foliage. Even where his body is partially masked by bushes with which his spotted hide may harmonise, his long neck is very likely to attract attention. We can scarcely consider that either the zebra or the giraffe is a case of "mimetism." The rough resemblance of the former to the royal tiger could have no deterrent effect upon lesser beasts of prey in a country like Africa, where the scourge of India is unknown. The general structure of the giraffe differs far too widely from that of the leopard to admit of the one being mistaken for the other. But we may even feel some doubt whether a concolourous animal is more easily seen than one that is striped, spotted, or clouded. No animal presents such a vast extent of self-coloured surface as does the elephant; yet all eastern sportsmen admit that, whether escaping or acting on the offensive, he has a wonderful power of remaining unseen at remarkably short distances. We must, therefore, I think, admit that in the colouration of the Mammalia there are points which cannot be explained by the doctrine of Natural Selection.

Passing to the great class of birds, we find, as regards colour, the most complete difference. In place of the poverty of tone and design characterising the Mammalia, we have here, probably, every conceivable colour, and patterns the most elaborate. Why such meagreness on the one hand, and such prodigality on the other? Is it in virtue of Natural Selection? We are certainly warranted in assuming that a coat of many colours may draw upon either bird or beast the observation of its enemies. We may think that the bird, having the power of flight, can more readily escape from its pursuers, and can thus—if we may use the expression—afford to be more showy. But in many gaily clad birds the power of flight is so limited as to afford them small protection against carnivorous beasts and reptiles. Birds, too, are more exposed to the attacks of eagles, hawks, and other predatory members of their own class, than are any—save the very smallest—beasts. So that we may well doubt if the feathered species do really enjoy any greater immunity from danger than do Mammalia. Nor must we overlook the fact that the winged mammals, the bats, are sufficiently sombre in their attire. Is it Sexual Selection? We may suppose that hen birds select mates of the richest plumage, whilst female beasts display no such preference. But then comes the question—Why is there such a difference of taste between the two great classes of warm-blooded vertebrates? To this question it is difficult to return any answer which is not substantially a reference to some inscrutable decree of the Creator. But there is the further difficulty that all birds do not display splendid hues and exquisite designs; many species are as dull and plain as the beasts of the field.

It is impossible to deny that the plumage of the Raptores bears, in colour and design, a manifest resemblance to the fur of the Felidæ. There are the same blacks and deep browns, on grey, buff, or stone-coloured grounds; there is the same general tendency to bars and spots. Now whilst admitting that patterns of this nature may aid in the concealment of cats lurking in a forest, they can have no such beneficial effect in hawks soaring in the open air, and seen in relief against the sky; nor can much stress be laid on the fact that this spotted plumage may withdraw female birds of prey from observation whilst sitting on their eggs. The nest is generally placed in situations not easily accessible to any creature except birds; and few of these, indeed, will care to disturb a hawk or eagle in the act of incubation.

Passing over the reptiles, amphibians, and fishes, let us next look at the distribution of colour among insects. Here we are met by puzzles not a few. Natural Selection, Sexual Selection, and Mimetism have indeed thrown a welcome—and often a startling—light upon many difficulties, but they have left at least as many unsolved. In butterflies, design may be said to have reached its summit. But to what end? It is impossible to conceive that all these exquisite and elaborate patterns—the admiration and envy of human artists—can serve in any way to promote the safety of the insect, or to secure its well-being; nor can we think that they are needed for the mutual attraction of the sexes. If it were really necessary for either purpose we might ask again, Why have butterflies—or at least Lepidoptera in general—the monopoly of this kind of beauty? For it is singular that, though colours even more brilliant occur among beetles and certain Heteroptera, and though there is in a lesser extent brilliance of hue among the remaining insect orders, down to the very Diptera,—the pariahs of annulose life,—yet in design the Lepidoptera stand in all Nature alone and unapproachable.

The colouration of beetles has also its difficulties. Among them we find metallic and iridescent shades in abundance, and in a perfection certainly rivalling, if not surpassing, those of the most splendid birds and butterflies. But the following tones are rare, if not altogether wanting:—Pinks, roses, lilacs, peaches, pale blues, pale greens, lavenders. On the other hand, deep reds, purples, violets, golden and coppery greens, deep blues (verging both to the violet and the green), yellows, and oranges abound. Elaborate patterns are rare, the nearest approach to the beauty of the butterflies in this direction being found among the Cicindelidæ, the Longicornes, and the Curculionidæ. But the colours in many species, even where not iridescent, shade gradually away from one tone to another.

It is scarcely possible, with any degree of plausibility, to harmonise these characteristic features with the laws of Natural or of Sexual Selection. In some cases the colours of a species seem exactly calculated to betray it to its enemies, or make known its approach to its intended prey. Take, for instance, *Calosoma sycophanta.* You see it running about, in the full light of day, on the bare, sandy soil of a pine-forest, or scaling the trunks of the trees in search of booty. Its broad elytra flash with golden-green, changing to an orange-scarlet, to which its blue-black thorax forms a striking contrast. It would be difficult to select an arrangement of colour which, under the circumstances, would be more conspicuous. Or, again, if a mass of the dung of the horse or cow be turned over, it will generally be found full of *Aphodii.* Some of these have reddish, some brownish, and others grey elytra,—all strongly reminding the observer of half-digested seeds. These colours must, therefore, attract the attention of domestic fowls and other birds which love to scratch and peck in dung. Yet these very species which thus attract the attention of their enemies are the most common. All these facts do not accord well with the law of Natural Selection. How, again, are we to explain the colours of the *Geotrupes* and *Onthophagi* of this country, and especially of the *Phanæi* of Mexico and Central America? These creatures fly by night, and in the day lurk chiefly in dung, or tunnel into the earth beneath it; yet they display a rich array of golden-greens, violets, purples, and bronzes.

We have already referred to the rarity or total absence of certain colours among beetles; yet to some of them—as the flower-haunting *Cetonias*—these very hues would seem desirable. Pink, lilac, rose, and peach-coloured flowers are numerous; why, then, are these colours so rare amongst honey-loving insects?

The predominant hues among the Hymenoptera are yellow, orange, brown, and black, or, on the other hand, deep metallic blue and purple. Why this limitation? These colours are scarcely the best adapted for the concealment of the insects amongst flowers and leaves.

But it is not merely in colour and design that animals exhibit characteristics not readily explained on the principles of Natural Selection and Sexual Selection. There are also peculiarities of structure. We must here especially call attention to the Dynastidæ. These huge beetles are remarkable for protuberances which in colloquial phrase are known as horns. The parts in question differ most essentially from the horns of Mammalia: they are integral portions of the skeleton, and, instead of being confined to the head, they are in many—if not in most—cases developments of the thorax, a part analogous to the shoulders and upper portion of the back in vertebrate animals. Hence they are incapable of motion, except in common with the entire body of the insect: they are so far sexual in character that they are almost confined to the males, and they vary exceedingly in magnitude among individual specimens of one species found in the same locality. If their presence is to be explained by Natural Selection they must have some reference to the habits and functions of the insect. But to what habits or functions? They are evidently not organs of sensation; as little can they serve in procuring food. For weapons—either against enemies of the same or of different species—they are not adapted, in some cases by reason of their position and direction, and in all by their very imperfect mobility. As to locomotion, they must be rather an impediment than an aid. Imagine an insect boring into mould or decayed wood, creeping under fallen trees, or through twigs and the roots of herbage, whilst encumbered with horns like those of *Hoplites Pan*, *Dynastes dichotomus*, or the *Golofæ*. In short, we are unable to conceive what purpose they serve. We find promiscuously individuals with highly developed horns, and others in which these parts are almost obsolete. Nor can we say that either kind appears to have any advantage over the other. If, *e.g.*, we examine a plot of ground in Central or Southern Europe where the spent bark from tanneries has been deposited, we shall find male specimens of *Oryctes nasicornis* with large and with small horns, respectively in about equal numbers. Now if any decided benefit accrued from either conformation we might expect that it would rapidly preponderate, and that the other form would, in the course of Natural Selection, have become rare, or be eliminated altogether. Similarly, if the large-horned males were regarded with especial favour by the females, we should anticipate that the small-horned type would gradually disappear by the process of Sexual Selection.

MacLeay, Swainson, and their followers, explain the horns of the Dynastidæ, or indeed of the entire Saprophagous Lamellicornes, on the principle that they represent the Ruminant Mammalia, and that, as the one group is horned, the other—whose function is to bury the excrements of the former—must be horned likewise. The analogy, however, is somewhat strained. The horns in one case differ signally from the horns in the other. The Dynastidæ—the most decidedly cornuted of all the Lamellicornes—do not remove or burrow in dung, and their metropolis is a region remarkably poor in ruminants. Still the idea of certain structural types reappearing, in a more or less modified form, in different sections of the organic world, may yet prove fruitful, and should not be entirely lost out of view. How it may best be harmonised with the doctrine of Descent is not a subject for present consideration.

Another difficulty for "Selection" is the existence of trimorphous species. Thus in *Papilio Memnon* there are two distinct forms of females, the one nearly resembling the male, and the other decidedly unlike. A single brood of larvæ, the issue of either kind, comprises at once males and specimens of both kinds of females. The males do not appear to show any exclusive preference for either form. On what principle has this anomaly arisen? Is it the initial step to the formation of two distinct species? If not, what end does it subserve that would not be equally well secured were the females all of one type?

Turning to a totally different point, we find it a general rule that—in the Vertebrata and Articulata—the sexual organs are situate at or near that extremity of the trunk which is farthest from the head. In spiders, however, it is asserted that these organs in the male are placed in one of the legs. Now, that this very peculiar arrangement is an advantage to male spiders we do not dispute. It is nothing uncommon for the female spider to destroy her mate, and hence a conformation which permits of intercourse without the close contact required in other forms of animal life may be beneficial to the male.* But by what process has such a change been brought about? We feel utterly unable to conceive even of the first stages of the needful transitions. It would seem that any male animal in which the generative organs were by some malformation removed from their usual site, even to a small extent, would be placed at a disadvantage, and be less likely to leave posterity than one of the normal structure. If we consider the intermediate positions through which the male organs of the spider must have gradually travelled in the course of myriads of generations, we shall be convinced that whatever advantages their present situation may afford, those points of transit must have rendered fecundation more dangerous and less easy. We should therefore expect that the line of descent in which these variations were occurring would—in the ordinary working of Natural Selection—tend to become extinct.

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* A somewhat similar arrangement prevails in the *Octopus*, which might almost be characterised as a sea-spider, though not ranking among the Articulata.

The distribution in the animal kingdom of the power of secreting poisons requires, also, much additional light. We may distinguish here two stages:—In the lower, poisonous fluids are indeed elaborated, or some of the solids of the body possess venomous properties, but the animal has no special apparatus for injecting them into the tissue of an enemy or of a victim. In the higher, there exist—in addition to the secreting glands and the receptacles for the poison—either hollow fangs, or stings, or some analogous weapon. In neither of these forms does the poison-faculty occur among Mammalia and birds, at least in a normal, healthy condition.* Among reptiles it seems restricted to one particular group, the Ophidians, where it receives its maximum development. Why, then, has no similar power been evolved among warm-blooded animals and among non-ophidian reptiles? Why, again, is the venom of serpents much more intense than is required for the destruction of their prey? The cobra feeds chiefly upon rats, but its bite is amply sufficient to cause the death of a man, a swine, or a sheep, which it is utterly unable to swallow. Nor, after all, can the venom of snakes be of great value to them for defensive purposes, since the bite of few species, if of any, is sufficiently rapid to prevent the bitten animal taking instant revenge. As, therefore, increased activity of venom can have conferred little especial advantage upon its possessors, either in procuring food or for the purpose of self-defence, it is difficult to see how its development can have been effected on the principle of Natural Selection. Still less can we conceive of it as due to Sexual Selection. The power, possessed as it is by both sexes, does not appear calculated to render either more attractive to the other.

Passing over the fishes, in some of which the venom-faculty exists in its higher stage of development, we come to the Articulata. Here true venoms exist in the Hymenoptera, Heteroptera, probably in certain Diptera, as well as in many spiders, scorpions, and centipedes.† On the other hand, the Neuroptera, Orthoptera, Homoptera, and Strepsiptera, are devoid of venom. In certain Coleoptera, as in the Bembidiidæ, in *Cychrus rostratus*, and a few other Carabidæ, formic acid can be ejected either as a liquid or as a vapour; but there is no special weapon for introducing it into any given object. Certain Lepidopterous larvæ have hairs, which if drawn into the lungs, or even brought in contact with the skin, produce painful and dangerous inflammation. But even if this effect is not solely due to the mechanical structure of the hairs, they are not under the control of the larva, and are distributed at random by the wind. There arise, then, the three following questions:—

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* Is hydrophobia, as occurring among wild Canidæ, such as wolves and jackals, invariably fatal? Its extreme frequency among those species leads us to doubt whether such is the case.

† The bite of the common English centipede, as the writer can testify, is more painful than the sting of a wasp.

1. Why is the poison-faculty developed in the particular groups above mentioned, and not in others?
2. Why is the poison-apparatus seated in the head, in bugs, spiders, and centipedes, but in wasps and scorpions in the abdomen or in the tail?
3. Are all the venom-bearing animals members of one and the same line of ascent? That they are seems impossible to be maintained.

Can we construct a pedigree in which all the poisonous groups can be arranged in order of filiation without the intervention of non-poisonous forms? Did some articulate type once exist from which the bugs, the wasps, the centipedes, spiders, and scorpions have all diverged, whilst non-poisonous Articulata sprang from another source? If we take our second question into consideration the matter is further complicated, as we shall then have to seek a separate ancestry for the bugs, centipedes, and spiders on the one hand, and for the wasps and scorpions on the other. If no such lines of descent exist, we have then to explain the independent appearance of this faculty at a number of distinct points in the animal kingdom, and among groups which cannot be shown to require it more than do several others not thus armed. A tiger-beetle, a dragon-fly, or a *Mantis*, able to inflict a poisonous bite, would, in the struggle for existence, have enjoyed a marked advantage over others of its kindred. If, then, the occurrence of the venom-faculty be due to Natural Selection, why is it found just where it at present exists, and not elsewhere? In the Hymenoptera we meet with further difficulties, in the sexual distribution of the poison-faculty, and in the fact that the organ which in some groups is used merely as an ovipositor, serves as a sting in others. In all species where the poison-power lies in the jaws the males and females are armed equally, as is also the case in the scorpion. We must, however, remember that the sting of the latter, though corresponding in function with that of the wasp or bee, is essentially different.

From the poison-faculty we pass to a power widely distributed among the Articulata, and even found in some Mollusca, but totally wanting among vertebrate animals—the power of secreting a textile matter and of forming it into threads and tissues. Most insects possess this power when in the state of larvæ, though in very different degrees of perfection. Among spiders only it exists in perfection in the adult state, and is used as a means of procuring food, or, we might say, as a weapon. Are, then, the silk-giving insects, arachnides, and mollusks* descendants of one common stock, or has it also originated independently in different groups? Here, again, it must be remembered that the spinning organs are not homologous. The spider emits its silk from certain abdominal appendages, whilst all insect larvæ spin from orifices in the face. The limitations of this power are also perplexing. Very useful, or rather necessary to many who possess it, it would have been equally useful to many who do not. What, then, is the reason that Natural Selection has operated just as we find it to have done? To many birds, and to certain arboreal mammalia, the power of spinning would have been of striking advantage. Yet the process which has evolved spinning spiders has failed to produce spinning birds and mammals. Who can solve this riddle?

Yet, again, we find in certain animals the power of phosphorescence—of emitting at will a distinct light. This faculty is, I believe, confined to the Articulata, and appears there in certain detached groups. It is found amongst Coleoptera, as in the well-known glowworm and fire-fly; amongst Homoptera, as in the lantern-fly, and in at least one centipede. But would it not have been equally useful to many other nocturnal animals, who might thus have attracted each other? If the digression is permissible it is curious why nocturnal mammalia are scared and repelled by a light, whilst nocturnal insects are universally attracted. What charm can the candle have for the moth? Nocturnal birds, also, have been known to dash themselves against the windows of lighthouses.

To return. Either all luminous animals are descended from one common stock, or the power of emitting light has been independently elaborated at distinct points, and not elsewhere—a repetition of the difficulty we have already encountered as regards the production of poison and of textile matter.

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* Pinna.

There is a strange phenomenon known as Melanism. In the more southern portions of the globe animal forms have a striking disposition to assume a black colour. This tendency is well marked in the temperate latitudes of South America, in South Africa and Madagascar, in Australia, New Zealand, and New Guinea, where it reaches the Equator. Perhaps the most familiar instance that could be cited is the black swan of Australia. New Zealand has a black parrot, and the high latitudes of South America a black humming-bird. Among insects it is well known that few large groups display a more brilliant general colouration than the Cetoniadæ, or rose-beetles, which have been not inaptly characterised as walking jewels. Yet in South Africa and Madagascar, where they are richly developed, they, too, appear in mourning. The genera *Stenotarsia*, *Liostraca*, *Heteroclita*, *Callipechis*, *Rhinocœta*, *Xiphoscelis*, and not a few others that might be mentioned, have scarcely a species which is not in great part black. Why this should be the case—while groups which elsewhere affect light or brilliant colours should be represented in the regions just mentioned by black members—is an unsolved question. Peculiarity of climate or of soil is out of the question, since the countries in which Melanism has been recognised include every conceivable variety of both, and since they form in these respects no exception to the remainder of the habitable globe. Can Natural Selection be the cause? Take the case of a black swan. Will his opportunities of surviving in the struggle for existence, and of leaving posterity behind him, be greater than if he had been white, like the swans of the northern hemisphere? We see no reason why this should be the case. But supposing, for argument's sake, that the black swan has the advantage, it might then be asked why he had not also been produced and perpetuated in other regions where swans occur. We want to find the law which connects the black swan with Australia, in preference to any other region of the world, and so far, Evolution leaves us as much in the dark as the old doctrine of distinct and special creation.

But Melanism is only one single instance of a general principle. Almost every country sufficiently large and sufficiently rich in animal and vegetable species, has its local characteristics. Place a miscellaneous, but local, assortment of insects before an experienced entomologist, and, though he may know nothing of them, and have never seen one of them before, he will from these characteristics suspect their origin. Now in this there is, to a certain point, nothing difficult to explain in accordance with the doctrine of Descent. Isolate two countries so that the animals and plants of each are perfectly excluded from the other, then, if species do undergo any variation in course of ages, we may naturally expect that the Faunæ and Floræ of the two regions will differ respectively from each other, and that the more decidedly the longer and the more complete has been their separation. As Mr. Wallace tersely puts the matter, "the individuality in the productions of a country is the measure of the time of its isolation." But here arises a difficulty; the variation in each zoological or botanical district is apt to take some one specific direction. Thus there are in America, if we remember rightly, some thirty vegetable species which all differ from their nearest representatives on the eastern side of the Atlantic in one and the same manner. Mr. Wallace remarks in the butterflies of Celebes, as compared with those of the adjacent islands, a peculiarity of outline. Their fore-wings are either strongly curved or bent near the base, or extremely elongated and hooked. Now the climate of Celebes differs very little, if at all, from that of Borneo on the west, or of the Moluccas on the east. It is difficult in the extreme to imagine any reason why butterflies with the peculiarities thus indicated should enjoy any advantage in the struggle for existence in Celebes which they would not equally experience in the surrounding islands. How, then, can we conceive these features due to Natural Selection? But there are local peculiarities, not merely in structure, but also in colour and design. To what can these be due? It needs but little effort of the imagination to recognise in the patterns of Chinese butterflies, moths, and beetles, something of the stiff and grotesque character which strikes us in Chinese decorative art.

Again, there is a difficulty in the distribution of zoological forms. Take those coleopterous groups which are most closely and directly dependent upon the vegetable kingdom, such as the Buprestidæ, Cetoniadæ, Dynastidæ, Curculionidæ, and Longicornes. We might naturally expect that in every tropical region where there is a profuse, luxuriant, and highly varied vegetable life, especially in the form of large trees, there the development of these groups would be mainly equal and similar. Yet nothing can be farther from the truth. There are decaying trees and rich vegetable mould in Africa, in India, and the Malay Archipelago. Yet the head-quarters of the Dynastidæ are unmistakably in South America. There are leaves and flowers in the warm regions of the western hemisphere, as well as in Africa, Southern Asia, and Australia. Yet the Cetoniadæ of the Old World are in round numbers four times as numerous as those of the New. Why have not they in Brazil, Guayana, Venezuela, and Mexico, developed themselves into as great a variety of forms as in Madagascar, the Guinea regions, the Cape, and India? It may be urged that their career in South America has been checked by enemies, as, for instance, by the destructive ants. But Africa is no less infested by these, our "six-legged rivals," than is South America. It is also difficult to see why any enemy which should interfere with the development of the Cetoniadæ, should not to a very serious extent interfere with the multiplication of Dynastidæ and Longicornes. To sum up this part of the subject it may be said we want to know what determines the variation of species, to take in different countries different directions? What connects certain forms of life with certain regions? Why do groups whose conditions of existence are the same—as far, at least, as our most careful scrutiny can discover—make their election of localities, and what determines their choice?

We cannot refrain from noticing here certain objections to "Darwinism," or rather to Evolutionism, which have been recently promulgated, but to which we can attach no great value. It is said that no monkeys are carnivorous, whilst all the stronger races of man are omnivorous, and even primarily carnivorous. It is a common error to lay great weight on the natural diet of an animal in proof of its affinities. We find the greatest diversity of diet in one and the same group. Thus whilst the polar bear, and, I believe, the grizzly (*U. Ferox*), may be considered exclusively carnivorous, there are other species of bear which decidedly prefer a vegetable diet. Among the Rodents we find some purely herbivorous, as the hare, the rabbit, and the Guinea-pig, and others which, like the rat, have a strong inclination for animal food. Nor must we forget that the earliest traditions we possess represent primeval man as a pure vegetarian.

A kindred objection is that the "nearest creatures to man in form are not the nearest in intellect. The elephant, dog, and horse, which have no affinity to man, have a far closer intellectual affinity than those pets of Darwinism, the gorilla and chimpanzee." If the author of these lines had mentioned the ant as having a "closer intellectual affinity to man" than any vertebrate animal, he would have commanded assent. We should then have reminded him that Evolutionism is the only theory of the animal kingdom with which such facts harmonise. All other systems have found it a severe shock that mental development did not keep a parallel course with structural affinity. Place a white ball at the top of every twig of a large tree: the second highest ball will not necessarily be found on the same branch that supports the highest of all. Mr. Darwin expressly declares that he does not consider man's place to be on the same ascending line as that to which the gorilla and chimpanzee belong. The anonymous writer quoted commits the common mistake of confounding docility and subservience with intellect. Could we enter into as close relations with the ape as we do with the dog, we should find the former far superior in intellect. But, like the higher races of man, he is independent in character, and does not take kindly to slavery. A female ape will drive away the flies from her sleeping infant. If it dies, she will often fret herself to death. Does a bitch ever show such an approach to human nature?

We have thus briefly glanced at a number of facts which still remain isolated and unorganised, and of which we are scarcely more able to give a rational account than were our forerunners two centuries ago. Our survey has been far from exhaustive, its purpose being to furnish, not a catalogue, but merely characteristic specimens. How, then, are such phenomena to be regarded? To ascribe them to chance is, substantially, what "the fool hath said in his heart." It may be asked why not simply refer them to the good pleasure of God, as did our fathers before us? We reply that it is equally irreverent and unphilosophical to conceive of the decrees of Absolute Reason as mere casual arrangements, based upon no fixed principles, and which might as well have been quite otherwise. Let us be sure that all these apparent anomalies are the outcome of laws —laws certainly difficult, and perhaps impossible, for us to comprehend. But it is at once our duty and our privilege to make the attempt.

VI. THE MECHANICAL ACTION OF LIGHT.

By WILLIAM CROOKES, F.R.S., &c.

SOME experiments illustrating the mechanical action of Light, which I have recently exhibited before the Fellows of the Royal Society, having attracted considerable attention, I propose to give here a description of some of the instruments which my researches have enabled me to construct. But, to render the subject more intelligible, it will be necessary to give a brief outline of the researches which I have been carrying on for the last three or four years, so that the reader may see the gradual steps which have led up to the full proof that Radiation is a motive power.

The experiments were first suggested by some observations made when weighing heavy pieces of glass apparatus in a chemical balance, enclosed in an iron case from which the air could be exhausted. When the substance weighed was of a temperature higher than that of the surrounding air and the weights, there appeared to be a variation of the force of gravitation. Experiments were thereupon instituted to render the action more sensible and to eliminate sources of error.[*]

My first experiments were performed with apparatus made on the principle of the balance. An exceedingly fine and light arm was delicately suspended in a glass tube by a double-pointed needle; and at the ends were affixed balls of various materials. Amongst the substances thus experimented on I may mention pith, glass, charcoal, wood, ivory, cork, selenium, platinum, silver, aluminium, magnesium, and various other metals.

The most delicate apparatus for general experiment was made with a straw beam having pith masses at the end. The general appearance of the apparatus is shown in Fig. 1.

A is the tube belonging to the Sprengel pump.[†] B is the desiccator, full of glass beads moistened with sulphuric acid. C is the tube containing the straw balance with pith ends: it is drawn out to a contracted neck at the end connected

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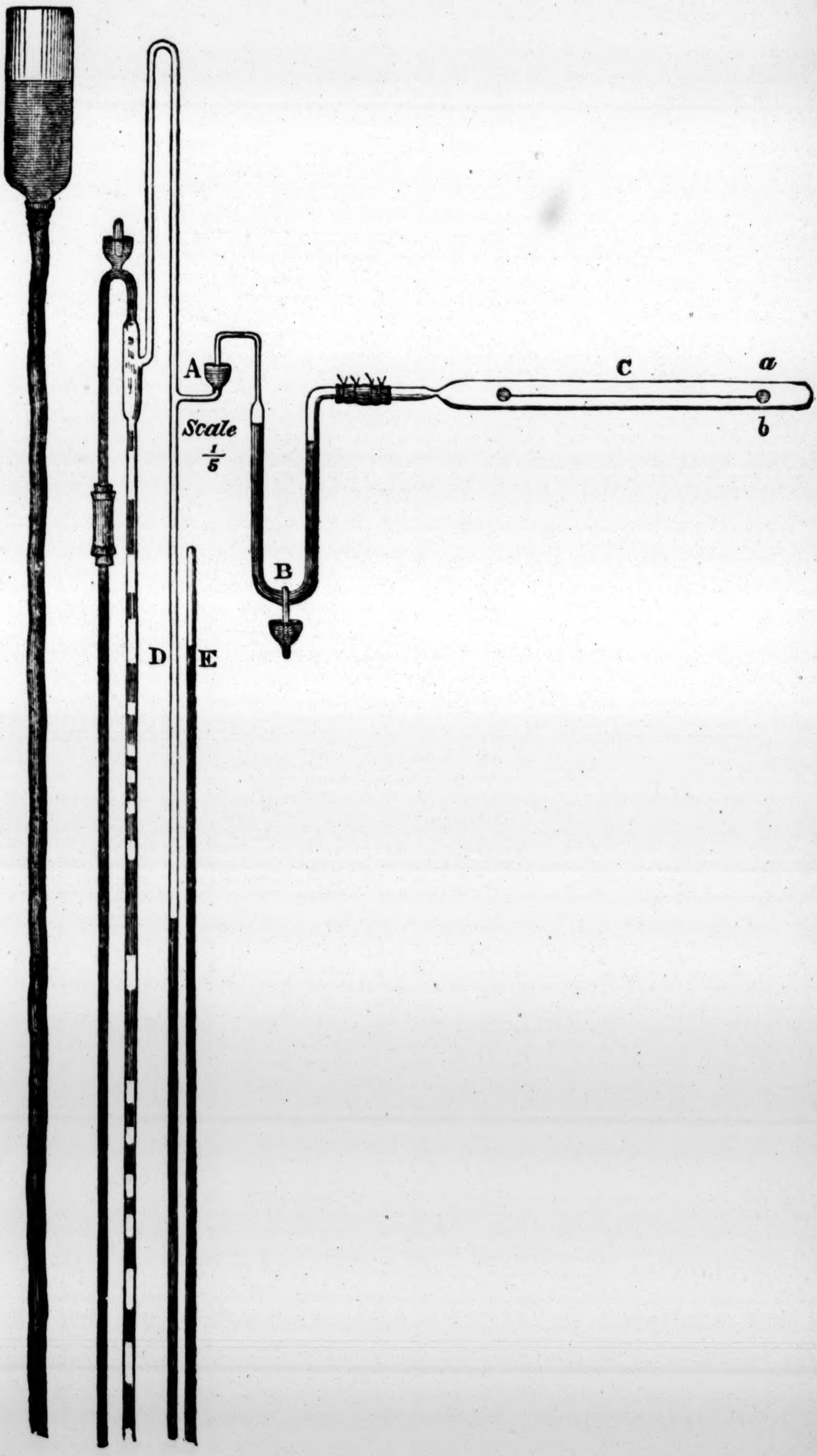
[*] "On the Atomic Weight of Thallium," Phil. Trans., 1873, vol. clxiii., p. 287.

[†] For a full description of this pump, with diagrams, see Phil. Trans., 1873, vol. clxiii., p. 295.

FIG. 1.

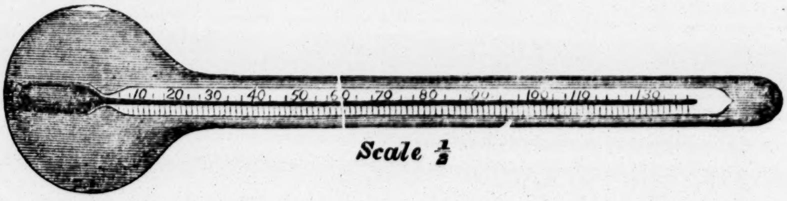


with the pump, so as to readily admit of being sealed off at any stage of the exhaustion. D is the pump-gauge, and E is the barometer.

The whole being fitted up as here shown, and the apparatus being full of air to begin with, I passed a spirit-flame across the lower part of the tube at *b*, observing the movement by a low-power micrometer: the pith-ball (*a b*) descended slightly, and then immediately rose to considerably above its original position. It seemed as if the true action of the heat was one of attraction, instantly overcome by ascending currents of air. A hot metal or glass rod and a tube of hot water applied beneath the pith ball at *b* produced the same effect as the flame; when applied above at *a* they produced a slight rising of the ball. The same effects take place when the hot body is applied to the other end of the balanced beam. In these cases air-currents are sufficient to explain the rising of the ball under the influence of heat.

In order to apply the heat in a more regular manner, a thermometer was inserted in a glass tube, having at its extremity a glass bulb about 1½ inches diameter; it was filled with water and then sealed up (see Fig. 2). This was arranged on a revolving stand, so that by means of a cord I could bring it to the desired position without moving the eye from the micrometer. The water was kept heated to 70° C., the temperature of the laboratory being about 15° C.

FIG. 2.



Scale ½

The barometer being at 767 millims. and the gauge at zero, the hot bulb was placed beneath the pith ball at *b*. The ball rose rapidly. The source of heat was then removed, and as soon as equilibrium was restored I placed the hot-water bulb above the pith ball at *a*, when it rose again, —more slowly, however, than when the heat was applied beneath it.

The pump was then set to work; and when the gauge was 147 millims. below the barometer, the experiment was tried again: a similar result, only more feeble, was obtained. The exhaustion was continued, stopping the pump from time to time to observe the effect of heat, when it was seen that the effect of the hot body regularly diminished as the rarefaction increased, until, when the gauge was about 12 millims. below the barometer, the action of the hot body was scarcely noticeable. At 10 millims. below it was still less; whilst when there was only a difference of 7 millims. between the barometer and the gauge, neither the hot-water bulb, the hot rod, nor the spirit-flame caused the ball to move in an appreciable degree.

The inference was almost irresistible that the rising of the pith was only due to currents of air, and that at this near approach to a vacuum the residual air was too highly rarefied to have power in its rising to overcome the inertia of the straw beam and the pith balls. A more delicate instrument would doubtless show traces of movement at a still nearer approach to a vacuum; but it seemed evident that when the last trace of air had been removed from the tube surrounding the balance—when the balance was suspended in empty space only—the pith ball would remain motionless, wherever the hot body were applied to it.

I continued exhausting. On next applying heat underneath, the result showed that I was far from having discovered the law governing these phenomena; the pith ball rose steadily, and without that hesitation which had been observed at lower rarefactions. With the gauge 3 millims. below the barometer, the ascension of the pith when a hot body was placed beneath it was equal to what it had been in air of ordinary density; whilst with the gauge and barometer level its upward movements were not only sharper than they had been in air, but they took place under the influence of far less heat—the finger, for example, instantly repelling the ball to its fullest extent.

To verify these unexpected results, air was gradually let into the apparatus, and observations were taken as the gauge sank. The same effects were produced in inverse order, the point of neutrality being when the gauge was about 7 millims. below a vacuum.

A piece of ice produced exactly the opposite effect to a hot body.

The presence of air having so marked an influence on the action of heat, an apparatus was fitted up in which the source of heat (a platinum spiral rendered incandescent by electricity) was inside the vacuum-tube instead of outside it as before; and the pith balls of the former apparatus were replaced by brass balls. By careful manipulation and turning the tube round, I could place the equipoised brass ball either over, under, or at the side of the source of heat. With this apparatus I tried many experiments, to ascertain more about the behaviour of the balance during the progress of the exhaustion, both below and above the point of no action, and also to ascertain the pressure corresponding with this critical point.

In one experiment, which is described in detail in my paper on this subject before the Royal Society,* the pump was worked until the gauge had risen to within 5 millims. of the barometric height. On arranging the ball above the spiral and making contact with the battery, the attraction was still strong, drawing the ball downwards a distance of 2 millims. The pump continuing to work, the gauge rose until it was within 1 millim. of the barometer. The attraction of the hot spiral for the ball was still evident, drawing it down when placed below it, and up when placed above it. The movement, however, was much less decided than before; and in spite of previous experience the inference was very strong that the attraction would gradually diminish until the vacuum was absolute, and that then, and not till then, the neutral point would be reached. Within 1 millim. of a vacuum there appeared to be no room for a change of sign.

The gauge rose until there was only ½ a millim. between it and the barometer. The metallic hammering heard when the rarefaction is close upon a vacuum commenced, and the falling mercury only occasionally took down a bubble of air. On turning on the battery current, there was the faintest possible movement of the brass ball (towards the spiral) in the direction of attraction.

The working of the pump was continued. On next making contact with the battery, no movement could be detected. The red-hot spiral neither attracted nor repelled. I had arrived at the critical point. On looking at the gauge I saw it was level with the barometer.

The pump was now kept at full work for an hour. The gauge did not rise perceptibly; but the metallic hammering sound increased in sharpness, and I could see that a bubble or two of air had been carried down. On igniting the spiral, I saw that the neutral point had been passed. The sign had changed, and the action was one of faint but unmistakable *repulsion*. The pump was still kept going, and an observation was taken from time to time during several hours. The repulsion continued to increase. The tubes of the pump were now washed out with oil of vitriol,* and the working was continued for an hour.

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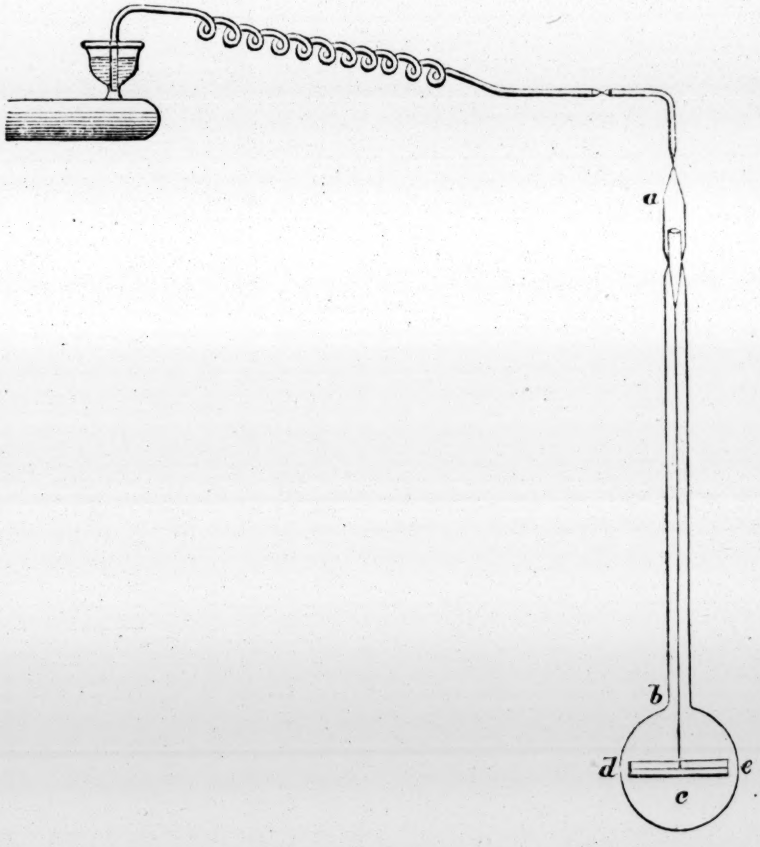
* Phil. Trans., 1874, vol. clxiv., p. 501.

The action of the incandescent spiral was now found to be energetically *repellent*, whether it was placed above or below the brass ball. The fingers exerted a repellent action, as did also a warm glass rod, a spirit-flame, and a piece of hot copper.

In order to decide once for all whether these actions really were due to air-currents, a form of apparatus was fitted up which—whilst it would settle the question indisputably—would at the same time be likely to afford information of much interest.

FIG. 3.



By chemical means I obtained in an apparatus a vacuum so nearly perfect that it would not carry a current from a Ruhmkorff's coil when connected with platinum wires sealed into the tube. In such a vacuum the repulsion by heat was still found to be decided and energetic.

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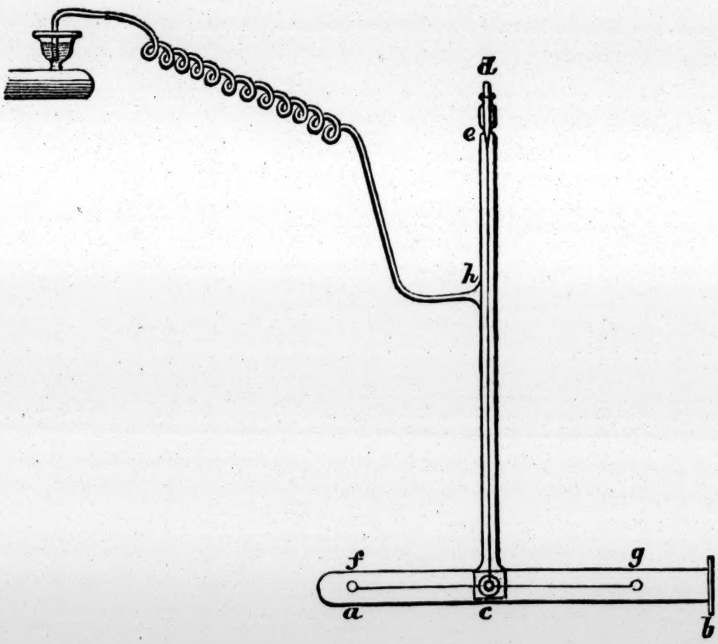
* This can be effected without interfering with the exhaustion.

I next tried experiments in which the rays of the sun, and then the different portions of the solar spectrum, were projected on to the delicately suspended pith-ball balance. *In vacuo* the repulsion by a beam of sunlight is so strong as to cause danger to the apparatus, and resembles that which would be produced by the physical impact of a material body.

A simpler form of the apparatus for exhibiting the phenomena of attraction in air and repulsion in a vacuum consists of a long glass tube, *a b* (Fig. 3), with a globe, *c*, at one end. A light index of pith, *d e*, is suspended in this globe by means of a cocoon fibre.

When the apparatus is full of air at ordinary pressure,

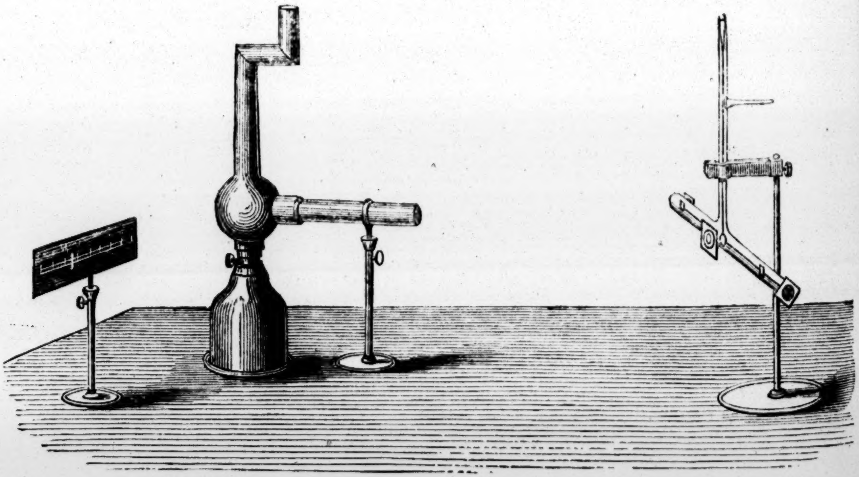
FIG. 4.



a ray of heat or light falling on one of the extremities of the bar of pith gives a movement indicating attraction. When the apparatus is exhausted until the barometric gauge shows a depression of 12 millims. below the barometer, neither attraction nor repulsion results when radiant light or heat falls on the pith, but when the vacuum is as good as the pump will produce strong repulsion is shown when radiation is allowed to fall on one end of the index. An apparatus of this kind constructed with the proper precautions, and sealed off when the vacuum is perfect, is so sensitive to heat that a touch with the finger on a part of the globe near one extremity of the pith will drive the index round over 90°, while it follows a piece of ice as a needle follows a magnet. With a large bulb, very well exhausted and containing a suspended bar of pith, a somewhat striking effect is produced when a lighted candle is placed about 2 inches from the globe. The pith bar commences to oscillate to and fro, the swing gradually increasing in amplitude until the dead centre is passed over, when several complete revolutions are made. The torsion of the suspending fibre now offers resistance to the revolutions, and the bar commences to turn in the opposite direction. This movement is kept up with great energy and regularity as long as the candle burns.

For more accurate experiments I prefer making the apparatus differently. Fig. 4 represents the best form. *a b* is a glass tube, to which is fused at right angles another narrower tube, *c d*; the vertical tube is slightly contracted at *c*, so as to prevent the solid stopper *d*—which just fits the bore of the tube—from falling down. The lower end of the stopper, *d e*, is drawn out to a point; and to this is cemented a fine glass thread about 0·001 inch diameter, or less, according to the torsion required.*

FIG. 5.



At the lower end of the glass thread an aluminium stirrup and a concave glass mirror are cemented, the stirrup being so arranged that it will hold a beam, *f g*, having masses of any desired material at the extremities. At *c* in the horizontal tube is a plate-glass window cemented on to the tube. At *b* is also a piece of plate glass cemented on.

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* Some of the glass fibres used in these torsion balances are so fine that when one end is held between the fingers the other portion floats about like a spider's thread, and frequently rises until it takes a vertical position.

Exhaustion is effected through a branch tube, *h*, projecting from the side of the upright tube. This is sealed by fusion to the spiral tube of the pump. The stopper *d e* and the glass plates *c* and *b* are well fastened with a cement of resin and bees'-wax.

The advantage of a glass-thread suspension is that the beam always comes back to its original position.

An instrument of this sort, perfectly exhausted and then sealed off, is shown at work in Fig. 5. It has pith plates at the extremities of the torsion beam. A ray of light from the lamp is thrown on to the central mirror, and thence reflected on to the graduated scale. The approach of a finger to either extremity of the beam causes the luminous index to travel several inches, showing repulsion. A piece of ice brought near causes the spot of light to travel as much in the opposite direction. In order to ensure the luminous index coming accurately back to zero, extreme precautions must be taken to keep all extraneous radiation from acting on the torsion-balance. The whole apparatus is closely packed round with a layer of cotton-wool about 6 inches thick, and outside this is arranged a double row of Winchester quart bottles filled with water, spaces only being left for the radiation to fall on the balance and for the index ray of light to get to and from the mirror.

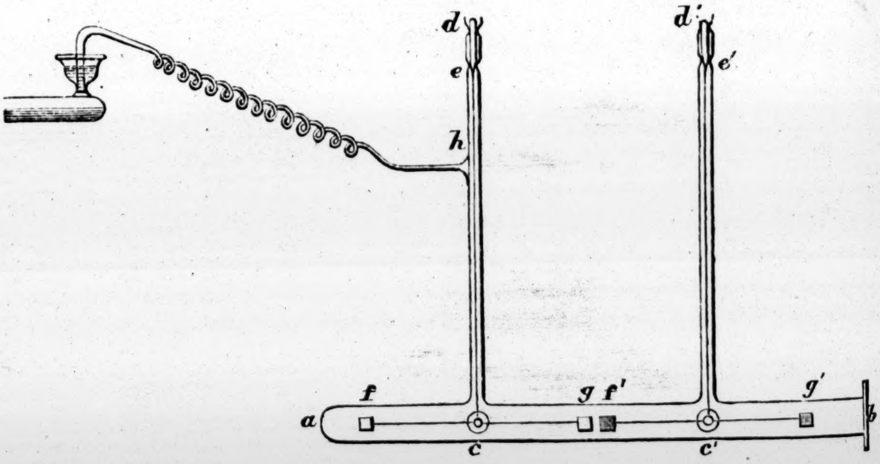
However much the results may vary when the vacuum is imperfect, with an apparatus of this kind they always agree among themselves when the residual gas is reduced to the minimum possible; and it is of no consequence what this residual gas is. Thus, starting with the apparatus full of various vapours and gases, such as air, carbonic acid, water, iodine, hydrogen, ammonia, &c., there is not found at the highest rarefaction, any difference in the results which can be traced to the residual gas. A hydrogen-vacuum appears the same as a water- or an iodine-vacuum.

The neutral point for a thin surface of pith being low, and that for a moderately thick piece of platinum being high, it follows that at a rarefaction intermediate between these two points pith will be repelled, and that platinum will be attracted by the same beam of radiation. This has been proved experimentally. An apparatus showing simultaneous attraction and repulsion by the same ray of light is illustrated in Fig. 6.

The pieces *f g* on the end of one beam consist of platinum-foil exposing a square centimetre of surface, whilst the extremities *f′ g′* on the other beam consist of pith plates of the same size. A wide beam of radiation thrown in the centre of the tube on to the plates $g f'$ causes g to be attracted and f' to be repelled, as shown by the light reflected from the mirrors, $c c'$. The atmospheric pressure in the apparatus is equal to about 40 millims. of mercury.

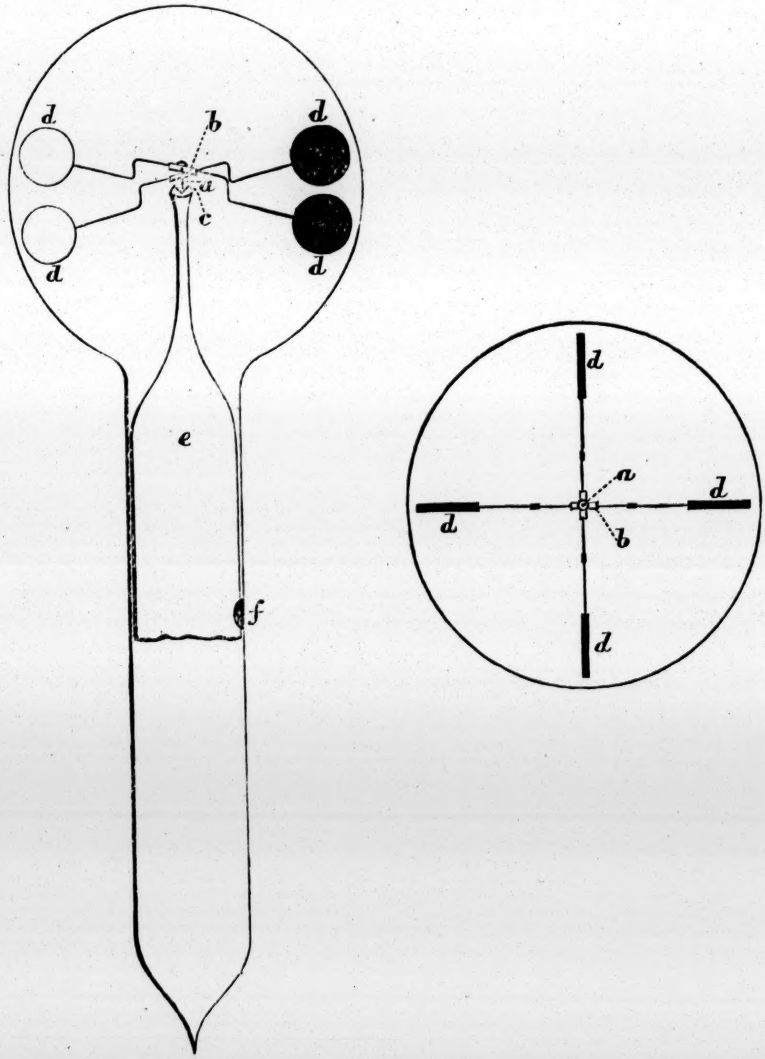
In a torsion apparatus similar to the one shown in Figs. 4 and 5 I have submitted variously coloured discs to the action of the different rays of the spectrum. The most striking results, as yet, have been obtained when the different rays of the spectrum were thrown on white and on black surfaces. The result was to show a decided difference between the action of light and of radiant heat. At the highest exhaustions dark heat from boiling water acts almost equally on white pith and on pith coated with lamp-black, repelling either with about the same force. The action of the luminous rays, however, is different. These repel the black surface more energetically than they do the white surface, and, consequently, if in such an apparatus as is shown at Fig. 4, one disc of pith is white and the other is black, an exposure of both of them to light of the same intensity will cause the torsion thread to twist round, owing to the difference of repulsion exerted on the black and the white surface. If, in the bulb apparatus shown in Fig. 3, the halves of the pith bar are alternately white and lamp-blacked, this differential action will produce rapid rotation in one direction, which keeps up until stopped by the torsion of the suspending fibre.

FIG. 6.



Taking advantage of this fact I have constructed an instrument which I have called the Radiometer, shown in section and plan at Figs. 7 and 8. It consists of four arms, of some light material, suspended on a hard steel point resting in a jewel cup, so that the arms are able to revolve horizontally upon the centre pivot, in the same manner as the arms of Dr. Robinson's anemometer revolve. To the extremity of each arm is fastened a thin disc of pith, white on one side and lamp-blacked on the other, the black surfaces of all the discs facing the same way. The whole is enclosed in a thin glass globe, which is then

FIGS. 7 and 8.



a. A very fine needle point.
b. Two pieces of straw.
c. Jewel cup.
d d d d. Four pith discs, blackened on one side. The arms between the straw in the centre and the discs are bent glass fibres.
e. Glass support holding cup.
f. Cement to keep the support *e* in its place.

exhausted to the highest attainable point and hermetically sealed.

The arms of this instrument rotate with more or less velocity under the action of radiation, the rapidity of revolution being directly proportional to the intensity of the incident rays. Placed in the sun or exposed to the light of burning magnesium, the rapidity is so great that the separate discs are lost in a circle of light. Exposed to a candle 20 inches off another instrument gave one revolution in 182 seconds; with the same candle placed at a distance of 10 inches off the result is one revolution in 45 seconds; and at 5 inches off one revolution was given in 11 seconds. Thus it is seen that the mechanical action of radiation is inversely proportional to the square of the distance. At the same distance 2 candles give exactly double, and 3 candles give three times, the velocity given by 1 candle, and so on up to 24 candles. A small Radiometer was found to revolve at the velocities shown in the following table, when exposed to the radiation of a standard candle 5 inches off.

Time required for One Revolution.

Source of Radiation.	Time in Seconds.
1 candle, 5 inches off, behind green glass . .	40
,, 5 ,, ,, blue ,, . .	38
,, 5 ,, ,, purple ,, . .	28
,, 5 ,, ,, orange ,, . .	26
,, 5 ,, ,, yellow ,, . .	21
,, 5 ,, ,, light red ,, . .	20

In diffused daylight the velocity was one revolution in from 1·7 seconds to 2·3 seconds, according to the intensity of the incident rays. In full sunshine, at 10 A.M., it revolved once in 0·3 second, and at 2 P.M. once in 0·25 second.

When heat is cut off by allowing the radiation to pass through a thick plate of alum, the velocity of rotation is somewhat slower, and when only dark heat is allowed to fall on the arms (as from a vessel of boiling water) no rotation whatever is produced.

In all respects, therefore, it is seen that the Radiometer gives indications in strict accordance with theory.

Several radiometers, of various constructions as regards details, but all depending on the above-named discovery, have been exhibited at the Royal Society, where their novelty and unexpected indications excited a considerable amount of interest.

This form of instrument is of too recent a construction for me to be able to do more than draw brief attention to a few of the many uses for which it is applicable.

By timing the revolutions of the instrument when exposed direct to a source of light—a candle, for instance—the total radiation is measured. If a screen of alum is now interposed, the influence of heat is almost entirely cut off, the velocity becomes proportionately less, and the instrument becomes a photometer. By its means photometry becomes much simplified; flames the most diverse may readily be compared between themselves or with other sources of light; a "standard candle" can now be defined as one which at x inches off causes the radiometer to perform y revolutions per minute, the values of x and y having previously been determined by comparison with some ascertained standard; and the statement that a gas-light is equal to so many candles may, with more accuracy, be replaced by saying that it produces so many revolutions.

To photographers the radiometer will be invaluable. As it will revolve behind the orange-coloured glass used for admitting light into the so-called dark room, it is only necessary to place one of these instruments in the window to enable the operator to see whether the light entering his room is likely to injure the sensitive surfaces there exposed; thus, having ascertained by experience that his plates are fogged, or his paper injured, when the revolutions exceed, say, ten a minute, he will take care to draw down an extra blind when the revolutions approach that number. Still more useful will the radiometer be in the photographic gallery. Placing an instrument near the sitter at the commencement of the day's operations, it is found that, to obtain a good negative, the lens must be uncovered—not for a particular number of seconds—but during the time required for the radiometer to make, say, twenty revolutions. For the remainder of the day, therefore, assuming his chemicals not to vary, the operator need not trouble himself about the variation of light; all he has to do is to watch the radiometer and expose for twenty revolutions, and his negatives will be of the same quality,* although at one time it may have taken five minutes, and at another not ten seconds, to perform the allotted number.

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* In this brief sketch I omit reference to the occasions in which the ultra violet rays diminish in a greater proportion than the other rays.

I have long been experimenting in the endeavour to trace some connection between the movements of attraction and repulsion above alluded to and the action of gravitation in Cavendish's celebrated experiment. The investigation is not sufficiently advanced to justify further details, but I will give here an outline of one of the results.

I find that a heavy metallic mass, when brought near a delicately suspended light ball, attracts or repels it under the following circumstances:—

I. *When the ball is in air of ordinary density.*

- *a.* If the mass is *colder* than the ball, it *repels* the ball.
- *b.* If the mass is *hotter* than the ball, it *attracts* the ball.

II. *When the ball is in a vacuum.*

- *a.* If the mass is *colder* than the ball, it *attracts* the ball.
- *b.* If the mass is *hotter* than the ball, it *repels* the ball.

The density of the medium surrounding the ball, the material of which the ball is made, and a very slight difference between the temperatures of the mass and the ball exert so strong an influence over the attractive and repulsive force, and it has been so difficult for me to eliminate all interfering actions of temperature, electricity, &c., that I have not yet been able to get distinct evidence of an independent force (not being of the nature of heat or light) urging the ball and the mass together.

Experiment has, however, shown me that, whilst the action is in one direction in dense air, and in the opposite direction in a vacuum, there is (as I have already pointed out in the experiments described in the commencement of this paper) an intermediate pressure at which differences of temperature appear to exert little or no interfering action. By experimenting at this critical pressure, and at the same time taking all the precautions which experience shows are necessary, it would seem that such an action as was obtained by Cavendish, Reich, and Baily should be rendered evident.

It is not unlikely that in the experiments here recorded may be found the key of some as yet unsolved problems in celestial mechanics. In the sun's radiation passing through the quasi vacuum of space we have the radial repulsive force, possessing successive propagation, required to account for the changes of form in the lighter matter of comets and nebulæ, and we may learn by that action, which is rapid and apparently fitful, to find the cause in those rapid bursts which take place in the central body of our system; but until we measure the force more exactly we shall be unable to say how much influence it may have in keeping the heavenly bodies at their respective distances.

So far as repulsion is concerned, we may argue from small things to great, from pieces of pith up to heavenly bodies; and we find that the repulsion shown between a cold and warm body will equally prevail, when for melting ice is substituted the cold surface of our atmospheric sea in space, for a lump of pith a celestial sphere, and for an artificial vacuum a stellar void.

Throughout the course of these investigations I have endeavoured to remain unfettered by the hasty adoption of a theory, which, in the early stages of an inquiry, must almost of necessity be erroneous. Some minds are so constituted that they seem impelled to form a theory on the slightest experimental basis. There is then great danger of their becoming advocates, and unconsciously favouring facts which seem to prove their preconceived ideas and neglecting others which might oppose their views. This is unfortunate, for the mind should always be free to exercise the judicial function, and give impartial weight to every phenomenon which is brought it. *Any* theory will account for *some* facts; but only the true explanation will satisfy *all* the conditions of the problem, and this cannot be said of any theory which has yet come to my mind.

My object at present is to ascertain facts, varying the conditions of each experiment so as to find out what are the necessary and what the accidental accompaniments of the phenomena. By working steadily in this manner, letting each group of experiments point out the direction for the next group, and following up as closely as possible, not only the main line of research, but also the little bye-lanes which often lead to the most valuable results, after a time the facts will group themselves together and tell their own tale; the conditions under which the phenomena invariably occur will give the laws; and the theory will follow without much difficulty. The eloquent language of Sir Humphry Davy contains valuable advice, although in terms somewhat exaggerated. He says,—"When I consider the variety of theories which may be formed on the slender foundation of one or two facts, I am convinced that it is the business of the true philosopher to avoid them all together. It is more laborious to accumulate facts than to reason concerning them; but one good experiment is of more value than the ingenuity of a brain like Newton's."

NOTICES OF BOOKS.

The Origin of Creation; or the Science of Matter and Force. A New System of Natural Philosophy. By THOMAS RODERICK FRASER, M.D., and ANDREW DEWAR. London: Longmans and Co.

To some of our contemporaries the book before us will supply, doubtless, matter for jokes not a few. To us it is simply a psychological phenomenon, and one of the most puzzling, as well as the most painful, that have come before us for many years. The case is this:—Either the great bulk of our knowledge in all the Sciences, from Astronomy to Biology, is a delusion, and the most eminent philosophers of the past and the present are mistaken, or this work is the most singular collection of errors and fallacies ever brought together by misplaced human ingenuity. If we, for argument's sake, admit the former alternative, we shall find it in the highest degree improbable that any two men within the limits of an ordinary life-time could acquire knowledge sufficient to demonstrate the fundamental errors of every branch of Science, and, going still further, replace these errors with truth. The Authors certainly lay claim to very extensive experience. The data upon which their "theories have been built have, in all the subjects touched upon, been based on personal observations in chemistry, telegraphy, and marine diving; in an extensive experience in coal and gold mines; also, while travelling in the Gulf Stream, the calms of the Equator, the coasts of Brazil, California, and Mexico, the Mediterranean, the Bay of Fundy, the hot sulphur-baths of Salt Lake, the great Geysers of California, and the mangroves of the Isthmus of Panama." During these researches and travels they profess to have discovered "the duality of atoms, the properties and force of matter, the cause of life, the source of mind, the cause of chemical action, the cause of sunlight, the cause of variation in ships' compasses, the cause of boiler explosions, the cause of winds and storms, the process of digestion, the cause of the tides, that magnetism is weight and supersedes gravitation, that coral is a semi-mineral growth and not the work of insects (!), the cause of meteors, the cause of auroras, the cause of the circulation of the blood, that hydrogen gas has the properties not only of metals but of minerals, that oxygen gas has the properties of vegetable matter." This we must admit is a very extensive and important array of discoveries, and a small part only, if substantiated, will suffice to earn for them the highest honours that Science has to bestow. We note, at the outset, that they desire to be judged in a rather peculiar manner:—"By the light of their faculty of common sense—the name, we may remark, under which many people worship their own ignorance—and their own personal observation, without reference to any book whatever, except it may be the Scriptures!"

The reader looking over such a list of discoveries and refutations will naturally expect that the book contains a detail of carefully conducted experiments and observations, from which the novel and startling views of the authors have been concluded. But there is nothing of the kind. We find assertion, but very little, if anything, worthy of the name of demonstration. The Authors do not argue; they preach. Whilst ignoring all other authority, they expect their own to be implicitly accepted.

Let us take, as a specimen of the work, the following passage:—"The mineral elements have naturally, as inherent elements, the cold colours, blue, black, and white; while the vegetable atoms are naturally possessed of the warm colours, red, yellow, and orange. Of course there appear to be exceptions. Gold is yellow, but it is very scarce, and is prized accordingly. Sulphur is yellow also, but it sheds a blue light when burned. Sometimes we see blue flowers also, but they are very rare indeed."

The last assertion is truly astounding. What English wood is not, in spring, blue over with the wild hyacinth? What cliff or moorland in the north is not gay in August with the blue harebell? What river in Derbyshire is not bordered with the blue forget-me-not? How many hill-sides in the Tyrol and Illyria—as Mr. Ruskin has eloquently described—are clad with the blue gentianella! A flax-field in flower is no very rare sight. If we turn from the fields and the woods to the garden we find the lupin, the campanula, the monk's-hood, the lobelia, the convolvulus (major and minor), the salvia, the crocus, and many others, all bearing blue flowers. Turning over the catalogue issued by an eminent London firm of nurserymen, we find the blue-flowering plants form one-ninth of the species therein included. As the flowers were classified into white, blue, purple, pink, crimson, scarlet, yellow, orange, and brown, the blue species hold numerically a fair average position.

Further, on the author's showing, white flowers ought also to be "very rare indeed." Yet they are, if possible, still more abundant than the blue: they accompany us all the year round, from the snowdrop of January to the Christmas rose of December.

Sulphur, the authors admit, "is yellow," although indisputably a mineral body. They seek, however, to dispose of this unpleasant fact by adding that "it sheds a blue light when burned." Now this is what, on their theory, it ought not to do. Sulphur, when burning, combines with oxygen one of their "vegetable elements," the characteristic colours of which we are told are "yellow, orange, and red." So that we might expect, on this view, that its flame would be red instead of blue. Iron and copper pyrites, which are decidedly not scarce, are yellow, and have frequently been mistaken for gold. Metallic copper is of an orange-red, but assumes various shades of green and blue when oxidised and dissolved in acids, which again clashes with the doctrines of this new philosophy. The compound of chromium containing the largest proportion of oxygen, perchromic acid, is of a deep blue. Here, therefore, Messrs. Fraser and Dewar are again at fault. It will not avail for them to say, "Of course there appear to be exceptions." Facts are not to be set aside so easily, and it is with facts, as we see, not with theories, that the views of the authors refuse to agree.

As might be expected, the authors indulge in an attack on Mr. Darwin's "Origin of Species," and fancy that they have refuted it. By way of compensation they believe in Abiogenesis, and take M. Pasteur to task for the imperfection of his experiments undertaken to refute the notion of spontaneous generation.

On p. 163 we read:—"It may account for the Scotch, as a people, being so little troubled with indigestion, that the food, especially of the poorer classes, is mainly composed of oatmeal, salt fish, and cheese." Is this said in earnest or by way of a joke? Most people find by experience that salt fish and cheese are decidedly indigestible,

The Authors promulgate, also, a system of medicine. Turning to Chapter xxix., in the hope of meeting with some new light thrown upon diseases and their remedies, we find merely a recommendation of hot water, purgatives, and "in urgent cases, an emetic." Without at all questioning the value of these agencies, we submit that they are nowise novel. Morison, the "Hygienist," long ago proclaimed "proper vegetable purgation" to be the remedy for every disease. The "life-pills," one and all, which, with such touching faith, the British public has bolted for the last half century, are purgatives likewise. What prospect of success remains, then, for Dr. Fraser and Mr. Dewar?

The Authors seem to believe that they have the misfortune to be "too far in advance of their day and generation." We should rather consider them as altogether off the right track—hopelessly and helplessly astray: they seem to us loose, hasty, careless observers, and random theorists. We do not deny that, owing to the imperfection of language, many of the statements here laid down might be long defended on paper; but verbal discussion is not the ordeal which scientific theories have in these days to undergo: they must legitimatise themselves by coordinating facts hitherto unexplained and anomalous, and by pointing the way to phenomena as yet undetected. If we may venture to suggest anything to authors who are on such excellent terms with themselves, we would advise them to take up some one subject out of the many broached in their work, and try what they can do with it. Let them prove, for instance, that colour is inherent in objects themselves, and does not depend on the light by which they happen to be illuminated. Or, turning from the sphere of theory to that of practice, let them complete their magnetic machine and demonstrate its superiority to the steam-engine. Let them thus strike a death-blow at the rampant "coal interests," and the only danger will be that the public will accept their hypotheses without asking for verification.

A Manual of Hygiene, Public and Private, and Compendium of Sanitary Laws. By C. A. Cameron, Ph.D., M.D., &c. Dublin: Hodges, Foster, and Co. London: Bailliere, Tindall, and Cox.

This work is certainly a valuable addition to our sanitary literature. It is addressed not merely to medical men, chemists, officials, and others who may have a special or professional interest in matters relative to public health, but to the community in general. As such we trust that it will powerfully aid in creating a sound public opinion on matters which, hitherto, in spite of much talk, have been practically neglected. Without wishing to play the part of alarmists, we must assert that this neglect is imprudent in the extreme. We are too much in the habit of assuming that those fearful outbreaks of pestilence which ravaged Europe in the Middle Ages are now out of the question. The Author, after giving a brief account of the dreadful plague of the fourteenth century, remarks:—"I wish to direct attention to these almost forgotten calamities, because they are calculated to teach us important lessons. Are we sure that we are safe from another visitation of the black death? There are epidemiologists who believe that the germs of this disease still linger amongst the deep valleys of the Himalayas, and that they may yet be wafted to Europe. If such an event should ever unfortunately take place, I fear that in some of our towns the virus of the disease would find a congenial soil." He states that the Legislature is taking active measure to improve the sanitary condition of towns. We fear that in this respect he forms a too favourable view, both of the present and the future. The recent Act, by which a new lease of life was granted to the London slaughter-houses, was certainly a step in the wrong direction. On the water-supply of towns we meet with much valuable information. Dr. Cameron very judiciously doubts the safety, as well as the economy, of Mr. Bailey Denton's plan of furnishing villages and small towns with good water by a system of shallow wells and pools. The Author considers that "in seasons of drought and heat the water of such shallow ponds as those proposed by Mr. Denton would become greatly deteriorated in quality."

Like all chemists who have given the matter a really fair and unprejudiced examination, Dr. Cameron considers that the process of Wanklyn and Chapman is the only practical mode of water analysis for sanitary purposes. But he points out that though waters containing much organic nitrogen are certainly bad, those containing little are not necessarily good. He mentions a public pump in Waterford, the water of which contained more than 4 grs. of ammonia and 3 of nitric acid per imperial gallon. The question is sometimes asked by the unthinking, why, if polluted waters are so injurious, our ancestors, who were more ignorant and careless of such matters than ourselves, were not entirely extirpated? The answer is at once apparent:—The pollution of water increases with the density of the population, —a process which is at this moment going on in a most striking manner in some of the New England states.

In speaking of the sewage question, Dr. Cameron remarks that the A B C process has failed. We fear that he makes this rash statement on the misleading authority of the late "Rivers' Pollution Commissioners." Had he taken the trouble to examine into the matter himself, he would have come to a very different conclusion.

We can most sincerely recommend this Manual to officers of health, public analysts, municipal corporations, boards of guardians, and, in short, to all whose duties involve a knowledge of sanitary conditions, sanitary reform, and sanitary legislation.

The Identity of Primitive Christianity and Modern Spiritualism. By EUGENE CROWELL, M.D. Vol. I. New York: G. W. Carleton and Co. London: Trübner and Co.

To pass any formal judgment upon this work, without having previously made Spiritualism the object of exclusive study, would be at once difficult and unfair. Not feeling so qualified, we must be content with attempting to furnish a brief exposition of the author's views, without either advocacy on the one hand or refutation on the other. There are here a certain number of phenomena. What is their explanation? By the majority of the educated world, including most men of science, all so-called materialists (apneumatists), and the bulk of practical "common-sense characters," they are set down, often with little inquiry, as a mixture of delusion and of conscious imposition. A small class, whilst believing that the subject has been greatly complicated both by fraud and by enthusiasm, hold that there still remains a substratum of facts which, if rightly dealt with, might point the way to natural laws of the profoundest significance,—perhaps to some modification of force far more nearly connected with the phenomena of life than is electricity or magnetism. A third party—among whom rank, perhaps, the majority of orthodox clergymen and the bulk of "religious society"—view the phenomena in question as real and authentic, but as infernal in their character and origin. Lastly, spiritualists, including the author of the book, ascribe the incidents described to the agency of disembodied human spirits.

Dr. Crowell addresses himself in particular to the third class, and seeks to combat their objections. He dedicates his book "To all liberal minds in the Christian churches who are disposed to welcome new light upon the spirituality of the Bible, even though it may proceed from an unorthodox source, and who dare weigh and consider, even though they may reject, the claim herein made for the unity of the higher teachings of modern Spiritualism with those of early Christianity." He contends that the revelations of Spiritualism are the great and needed remedy against the development of Materialism, against which the Church has been powerless, on account of her unbelief in "spiritual gifts." These gifts, claimed by the early Christians, he urges, have not ceased, as Protestants suppose, but are still exercised. Spiritualism, therefore, is no novelty, but merely a re-manifestation of powers familiar to the first teachers of Christianity, and which, though obscured and neglected in succeeding centuries, has never ceased to exist. "I shall attempt," he writes, "to prove the genuine character of the so-called miracles of the Bible by evidence as strong as that required to decide the most important cases in our courts of law, by establishing the fact of the occurrence of similar miracles in our day constantly occurring in our midst, and which may be witnessed by all, and have been witnessed by thousands of persons of greater intelligence than most of those who witnessed the Bible miracles, and upon whose testimony these depend for credibility; and while I contend for equal credibility for both those which are recorded in the Bible and those which are now occurring, I shall be able to show that the different manifestations at the present time are fully as wonderful as those in ancient times, and that whereas they were little understood then, they are far better understood now."

As arguments against the asserted diabolical origin of Spiritualism, he contends that it has effected numerous conversions from Atheism, from Deism to Christianity, and from Unitarianism to orthodoxy. He maintains that Spiritualism has an elevating and purifying effect upon the inner life, and proclaims it to be simply "Christianity minus the framework of the ecclesiastical structure,—that is, Christianity stripped of the terrors with which superstition and error have invested it."

In support of these views Dr. Crowell displays great learning and research, and no small degree of acumen. Of his thorough sincerity the work seems, to us, to contain abundant evidence. But in how far his interpretation of certain passages of the Old and New Testaments will be admitted, by competent authorities, we entertain some doubt. Nor do we think that his comparatively low estimate of the Old Testament will greatly aid him in his attempts at reconciling Spiritualism with Christianity.

The whole question is one which obviously demands a broader, a more profound, and a calmer investigation than it has yet received. We fail to see what good end can be served by refusing such an inquiry, and by ridiculing scientific men who approach it in an impartial spirit.

The Blowpipe: a Guide to its Use in the Determination of Salts and Minerals. Compiled from Various Sources, by G. W. PLYMPTON. New York: D. Van Nostrand. London: Trübner.

THIS manual does not profess to be an original work. The first two parts, as we are told in the preface, "have been adapted, with but few emendations, from the work of Sheerer and Blanford. The alterations have chiefly been in the chemical symbols, the new nomenclature replacing the old." Symbols, of course, are invaluable where decompositions have to be explained, but we certainly do not see their utility in analytical manuals. When sesquioxide of chromium is mentioned, the Author's meaning would be fully understood without the addition of (Cr_2O_3).

The third part of the work is said to be translated from Gueront's "Guide Pratique pour la Détermination des Minéraux," which was originally written by Fuchs, of Heidelberg. We do not perceive anything erroneous or objectionable in the instructions given, and we have no doubt that the work will prove useful to students.

A Manual of Metallurgy. By WILLIAM HENRY GREENWOOD, F.C.S., &c. Vol. I.—Fuel, Iron, Steel, Tin, Antimony, Arsenic, Bismuth, and Platinum. Illustrated by 59 Engravings. London and Glasgow: William Collins, Sons, and Co. 1874.

AMONG the various branches of pure and applied Science in which the Department of Science and Art hold annual examinations, Metallurgy has for many years held a place, although the number of candidates for examination on this subject is, as might be expected, invariably small. Indeed it is difficult to see how the student can obtain the necessary information on this branch of Science, since our standard works on Metallurgy are, for the most part, beyond the reach of the class of students with whom the South Kensington system chiefly deals. It was therefore highly desirable that a cheap and trustworthy text-book should be compiled, and Messrs. Collins have done well to include a Manual of Metallurgy in their "Advanced Science Series." Mr. Greenwood has written the work with the view of meeting the requirements of the Kensington syllabus, and has succeeded in producing an extremely serviceable little Manual. Of course there is no room in so elementary a work for any original treatment of the subject, but the writer gives a succinct analysis of what others have written, and has evidently profited by his training under one of the best metallurgists in this country. After defining the terms peculiar to Metallurgy, Mr. Greenwood devotes a chapter to Fuel, and then passes to the study of Iron and Steel, which forms, indeed, the bulk of the book. The minor metals—tin, antimony, arsenic, bismuth, and platinum—are then briefly disposed of; but we fail to see what principles of classification have induced the writer to thus bring these rarer metals into juxtaposition with iron. The modern chemical notation, nomenclature, and atomic weights are employed, and the information generally is brought up to date. But although thus satisfactory as a whole, it would, of course, be easy enough to find fault with certain parts of the work. It is to be regretted that wherever German expressions are employed they are always bungled. Thus the writer cites, in his preface, "Bruno Kerl's *Handbuch der Metallurgischen" (sic)*; again, we are surprised to find the common word *Spiegeleisen* incorrectly spelt wherever it occurs. But trivial typographical errors of this kind, although annoying to an observant reader, do not, after all, detract much from the merits of the Manual. Indeed we hope that Mr. Greenwood will be as successful in his second volume—a volume which will deal with the remaining metals—as he has been, on the whole, in the present instalment of his work.

The Best Mining Machinery; an Essay. By RALPH GOLDSWORTHY. Reprinted, with Alterations and Additions, from the Royal Cornwall Polytechnic Society's 41st Annual Report, 1873.

ORIGINALLY printed in the "Mining Journal," and afterwards in the "Report of the Cornwall Polytechnic Society," this Essay is already known to a large number of those who are interested in mining. Nevertheless it is of so useful a character as to fully deserve a separate issue. Mr. Goldsworthy brings a good deal of practical knowledge to bear upon his subject, and gives the reader the benefit of a wide experience. It is a thoughtful and suggestive Essay, which will be read with interest by all who are connected with metalliferous mines.

Number, a Link between Divine Intelligence and Human: an Argument. By C. GIRDLESTONE, M.A. London: Longmans and Co.

THE object of this little work is to show that "man is made in the likeness of his Maker, instead of being, as surmised by some, developed from the lower orders of creation." We may remark, parenthetically, that the conditions here given as two alternatives are by no means mutually exclusive. Man may have reached the likeness of his Maker quite as easily by a process of evolution as by a direct, and, so to speak, mere mechanical mode of creation. "It is to the bearing of Number on this question of man's lineage, as pointing to an origin for the human race not below human nature, but above it, that the following argument will chiefly direct attention; pursuing throughout one definite chain of reasoning, which it is believed has not been elsewhere so distinctly set forth. And inasmuch as it rests mainly on grounds which underlie the foundations of modern Science, in most if not in all of its departments, it may perhaps have some little weight with those who follow after knowledge scientifically, out of a pure desire to discover and hold fast that which is true."

The Author's argument runs thus:—In the universe there appear certain numerical relations, which man is able to apprehend. Hence he may be assumed to bear a resemblance to the Creator of the universe. On the other hand, asserts the author, the lower animals are devoid of the faculty of number. Hence they differ from man not in degree, but in kind, and, as a consequence, man cannot have been evolved from any lower form.

We have thus, we believe, fairly stated the Author's train of reasoning, and have now to examine its validity. That there appear in the universe numerical relations, or at least uniformities, which man interprets and apprehends by number, no one will dispute. That man has, though to a very varying degree, the power "to calculate number and to apprehend its intimate relations" is also evident. But who tells us "that no (like) traces of a faculty to apprehend relations of Number have been met with in bird or beast is a proposition which may be confidently maintained"? This the Author has no right to take for granted. To give an illustration:—We stand before an audience, and hold up two minerals. "These," we say, "have a certain resemblance, but they differ herein that the one contains arsenic whilst the other does not." But if it turns out that we have not analysed the latter, but have merely assumed the absence of arsenic, we shall stand confounded. It is all very well for the Author to say "No one of these creatures, *it may be safely averred*, can discern the presence of number in things around it." Until the mental manifestations of animals have been investigated far more closely and impartially than they have been, it is the extreme of rashness to make any such statement.

But we can go further: animals do discern the presence of number in things around them. The Rev. J. G. Wood, in his interesting work "Man and Beast," gives a case of a dog whom his master, when drunk, had cruelly and unjustly beaten. The dog, after this, watched his master's potations, and as soon as it saw him take the fourth glass it invariably hid itself, and kept out of the way till he was sober.

Another instance, still more decisive, is the following:—A man wishing to shoot a crow which had her nest in a hollow near the top of a high tree, concealed himself in an outhouse close at hand, and waited for her to fly out. She remained concealed, however, till he had gone away. He then, to puzzle her, got another man to come with him into the shed, and go away after a time. Still she refused to come off her nest. Larger numbers of men were then tried; but it was not until seven men had gone in and six came away, leaving one behind, that her arithmetic failed her, and she was shot. Bitches, whose puppies have been removed and brought back, have been observed examining the lot with a puzzled air, as if in doubt whether the whole brood had been returned. The same phenomenon has been remarked with cats and their kittens. These facts, we submit, prove that animals can, though but to a small extent, recognise number. The crow saw that six was greater than five. The dog could count up to four, or, in other words, could perform a simple addition sum to that extent. This is very little less than what we observe among savages, some of whom can barely count as far as seven. The difference between man and beast, as far as number is concerned, is therefore one of degree, not of kind, and Mr. Girdlestone's argument must be set aside as null and void.

The Author seems not "to conceive it possible that quadrupeds might, in the course of long ages, become bipeds." Were there no mammalian forms on earth, living or fossil, save Hodge the ploughboy and Dobbin his horse, some difficulty might be experienced. But if we survey the whole mammalian series we perceive that the nearer we approach man the more the anterior extremities lose the character and functions of legs, and assume those of arms. Place side by side the arm of Hodge, the anterior limb of a gorilla and the fore leg of Dobbin, and no one can deny that the two former terminate in hands, and that the difference between them is trifling indeed compared to the contrast between the two latter.

Whilst holding, however, that Mr. Girdlestone has signally failed in proving his case, we consider that he deserves the thanks of naturalists for having drawn attention to a point which in their studies of animal intelligence has been too much neglected.

An Experimental Inquiry into the Nutrition of Animal Tissues. By WILLIAM MARCET, M.D., F.R.S. London: Longmans and Co.

THE nature of this interesting memoir may best be shown by a brief sketch of the conclusions at which the author has arrived. He holds that there is a safe ground for believing that the elementary constitution of muscle and other animal tissues is similar to that of a jelly, with this distinction—that its fibrinous or cellular form gives it due tenacity for the performance of its functions, but its water, albumen, and other constituents, hold the same physical relation as would water to gelatine in jelly. All tissues are formed of three different classes of substances,—those which constitute the ripe tissue, or the portion insoluble in water; those constituting the nutritive material of the tissues which are soluble in water, and *colloid*; and, lastly, the *effete* material which is also soluble but *crystalloid*, and consequently diffusible. In chemical composition the nutritive material and mature tissue do not differ, the change being simply morphological, and consisting in the assumption of an organised form. The whole of the phosphoric acid in muscular tissue is eliminated either as neutral tribasic phosphate or as pyrophosphate of potash. In flesh there exist, however, certain amounts both of phosphoric acid and of potash, which are not in the proportions of any known phosphate, and which take part exclusively in the actual formation of the mature tissue. The albuminous constituents of muscular tissue appear to be eliminated in the process of waste, in the state of kreatin, kreatinin, and other crystalloid bodies. Blood yields to flesh considerably more potash than is actually required in the formation of muscular tissue, the excess serving to eliminate the phosphoric acid by converting it into a crystalloid salt. In the lungs, however, Mr. Marcet thinks there is good reason to believe that the potash is eliminated, not exclusively as phosphate, but to a great extent as a crystalloid carbonate, in consequence of the carbonic acid emitted from the blood whilst passing through the lungs. The *effete* material in the muscles contains phosphoric acid and potash, in the respective proportions of 43 and 57; that in the lungs in the proportion of 11·32 to 88·68.

He finds that wheat flour, potato, and rice contain certain amounts of colloid phosphoric acid and colloid potash, which in these three kinds of vegetables exist nearly in the proportions of 1 part of total phosphoric acid to 0·55 of colloid phosphoric acid, and 1 part of total potash to 0·24 of colloid potash. This statement is based on the following experiment:—"100 grammes of wheat flour were mixed with enough distilled water for the whole to be nearly liquid: this was placed in a dialyser, which was floated for 24 hours over a bulk of water equal to 8 or 10 times that of the contents of the dialyser; the volumes of the contents of the dialyser and of the solution outside were then determined.

The material in the dialyser was next dried and incinerated, and the ash analysed for the determination of the phosphoric acid and potash. On the other hand, a certain quantity of flour was carefully incinerated, and the phosphoric acid and potash were determined in the ash. A correction had to be introduced into the analysis by diffusion, owing to the colloid mass still holding a proportion of diffusible phosphoric acid and potash, depending on the relation existing between the volumes of fluid in and out of the dialyser."

We fear that this process is open to a serious objection. Analysis of the material in the dialyser will show not merely the phosphoric acid, soluble, though in a colloid state, but also whatever phosphoric acid may exist in a state insoluble in water, such as the phosphates of lime and magnesia. Before assuming any of the residual phosphoric acid to be in a colloid state, it seems necessary to show that it is, in part at least, soluble.

Passing over the Author's views on the modifications of the nutritive process in phthisis, we come to what may be regarded as the final summary of his investigations:—"That in Nature soluble matter is undergoing a perpetual transformation,—passing in rotation from the crystalloid into the colloid, and from the colloid into the crystalloid condition. Animal secretions and the products of decomposition of animal and vegetable tissues are crystalloid, admitting of their ready distribution through land and water by a physical process of diffusion. These crystalloid substances are transformed into colloids by plants, and used in that form as food for animals; and both plants and animals yield them back again in their original crystalloid condition. Chloride of sodium alone appears to be an exception to this rule."

To a very considerable extent this generalisation must be regarded as well founded. The chief constituents of food—albumen, fibrin, casein, gelatin, glucose—are undoubtedly colloid. Lactose, indeed, which is a truly crystalloid substance, is a necessary of life to the young of all mammalian animals. On the other hand, the excretions are rich in compounds admittedly crystalline.

La Terre Vegetale. Geologie Agricole. Par STANISLAS MEUNIER, Paris: J. Rothschild.

WE have here a terse, plain, and lucid manual of agricultural geology, with especial reference to the nature and formation of the vegetable soil. The main materials of mould—sand, clay, lime, and humus—are described, and their functions pointed out. Plain instructions are given for a physical analysis of soils, followed by a description of the various types of mould, such as the loamy, the clayey, &c

In the second part the Author explains the formations of soils, which he divides into *local*,—*i.e.*, such as are produced on the spot where they are found by the decomposition of rocks,—and *soils of transport*,—the deltas, polders, and warps brought down by rivers, and deposited at their mouths or along their banks. In some parts of the world, especially in Mexico and the island of Candia, transported soils are met with which appear due to the action of wind rather than to that of water.

Turning from theory to practice, the Author next treats of the amelioration of soils. He follows the useful French custom of dividing the substances which the farmer may have occasion to add to his fields into two great classes, amendments or correctives, and manures, properly so-called. Under the former head he ranks lime, marl, clay, and even water, whether the latter is added by irrigation or withdrawn by drainage. The circumstances under which these operations are necessary, and the effects produced, are well described. The proportion of lime used in France appears to be only one-fourth to one-fifth of the quantity applied to the same surface in England. Irrigation, as the Author judiciously points out, produces more beneficial effects in Southern France and in Italy than in colder and moister climates. On the other hand, it is in the latter that the results of drainage are most conspicuous. In treating of mineral manures the Author reminds his readers of a truth too often overlooked,—*i.e.*, that a manure cannot prove uniformly and equally beneficial upon all soils and under all circumstances. Thus, a phosphatic manure will be found useless in situations where the sub-soil yields a supply of phosphoric acid equal or superior to the amount annually withdrawn by the crops.

A valuable feature of the work is an agricultural map of France, drawn up by M. A. Delesse, Professor of Agriculture at the Ecole des Mines. This map shows not merely the respective amounts of land under the plough, and occupied by vines, forests, and meadows, but it indicates the average returns per hectare in different parts of the country. It appears that only 6 per cent of the land in France yields a net return exceeding 80 francs per hectare; 10 per cent yields revenues ranging from 80 to 60 francs per hectare; in 20 per cent the returns range from 60 to 40; in 44 per cent the yield is comprised between 40 and 20 francs per hectare; whilst in 20 per cent the proceeds of cultivation range between 20 francs and nothing. If we consider that a hectare is, practically speaking, 2½ statute English acres, we must admit that there still remains great room for improvement. The woodlands are stated as yielding a nett average return of 20 francs per hectare; arable lands, 42; vineyards, 69; meadows and pastures, 72; and gardens, 120.

This work is a valuable addition to the series of agricultural and horticultural manuals with which the name of the publisher is honourably associated.

Report of the Commissioners of Agriculture for the Year 1872. Washington: Government Printing Office.

WITHOUT pleading guilty to a wish to "Americanise" our institutions, we cannot help pointing out, as a feature worthy of imitation, the accessibility in the United States of all kinds of official reports, whether of Government departments, municipal boards, &c. Documents of such kinds are to be met with in all literary institutions, public libraries, &c., and fall into the hands of every newspaper editor. As a consequence, the valuable information which they contain is disseminated over the whole country.

The volume before us contains the statistical reports of the principal crops in every State of the Union, showing the total amount of crop, average per acre, number of acres in each crop, value per bushel, ton, or pound, and total valuation. There is a Report on the forests of the United States. Contrary to general opinion, the area of forest bears a smaller proportion to the total area of the country in the United States than in Russia, Norway, Sweden, and probably in Germany. Reports of the exports of agricultural produce are given in great detail.

The Report of the Entomologist and Curator of the Museum gives very interesting facts concerning the ravages of *Anarsia lineatella*, *Aræocerus prunella*, *Romalea microptera*, and other of the insect pests with which the American farmers are greatly exercised. A decoction of the berries and leaves of the pride of China (*Melia azedarach*) has been recommended as an insecticide, but no mention is made of *Pyrethrum roseum* and *carneum*, so much extolled in some parts of the world as the sovereign remedy for mosquitoes and sand-flies.

The Chemist to the Department gives the analysis of a number of natural fertilisers, among which we may mention a marl from Glymont, Maryland, containing 4·41 per cent of tribasic phosphate of lime, and another phosphatic marl from Charleston, containing 16·34 per cent of insoluble, besides 1·38 per cent of soluble, phosphoric acid. The "poison-soils" of Dallas county, Texas, are characterised by the entire absence of sulphur-compounds and the large proportion of humus in insoluble states. Cotton, fruit-trees, and root-crops invariably perish if planted in this soil. The chemist suggests, in our opinion quite correctly, thorough under-drainage, sub-soil breaking, and a heavy dressing of lime and gypsum. The paper on the refuse of cities and towns is worth careful perusal, trite as the subject has become. Mention is made of the pneumatic system of Liernur as worthy of more careful attention than it has yet received.

The work throughout is a perfect store-house of valuable information for all who are directly or indirectly interested in agriculture.

Report of the Sanitary Committee of the Board of Health on the Concentration and Regulation of the Business of Slaughtering Animals in the City of New York. New York: D. Appleton and Co.

We learn from this Report that New York, like London, suffers from the nuisance of intramural slaughter-houses. There are, in one district of the city, fifty-four of these establishments, "for the most part of the cheapest construction; the yards roughly paved, the sewerage imperfect, and the means of cleansing of the most imperfect character." As a necessary consequence there are, in the immediate neighbourhood, "large numbers of establishments devoted to fat-melting, lard-rendering, hide-curing, gut-cleaning, tripe-curing, glue-making," and other unsavoury trades. The reason why these *foci* of fever and zymotic disease are thus concentrated in a certain district is one from which we, in London, may learn an important lesson. "The slaughter-houses have occupied the area described since 1868, when they were removed from above 40th Street by the Metropolitan Board of Health. At that time this section of the city was sparsely settled, and the Board allowed them to locate above the street described without regard to the inevitable wants of the rapidly increasing population." Hence those interested in the nuisance now assert that "having removed their business above 40th Street in obedience to the orders of the Board of Health, in 1868, and having permits to occupy their present locations, this Board is bound in good faith not again to remove or disturb the business of the remonstrants." Is not this exactly analogous to the position of London as regards the burial of the dead? The old City churchyards have been closed, and the suburban cemeteries, foolishly permitted to be laid out in elevated localities, are fast becoming an equal nuisance.

The New York Sanitary Committee propose the abolition of private slaughter-houses *in toto*, and the opening of public abattoirs, where all cattle will be slaughtered under official inspection. Such a step, in addition to its direct sanitary action, would abolish the driving of infuriated oxen through crowded streets, needless cruelty in slaughtering, the introduction of diseased and putrid meat into the markets, and a long array of subsidiary evils. We fear that London is farther from reform in this point than New York. Recent legislation seems to have given a new lease to these nuisances.

The Safe Use of Steam, containing Rules for the Guidance of Unprofessional Steam-Users. By An Engineer. London: Lockwood and Co. 1874.

In plain-spoken words the writer, as a practical engineer, offers wholesome advice to all who have the care of steam-boilers and the control of steam-power. There can be little doubt that due attention to such rules as those laid down in this little work would go far to prevent boiler-explosions, a class of accidents which in most cases may be traced to the ignorance or recklessness of those who are in charge. "Be slow to believe, with some, that explosions occur from mysterious and unpreventible causes. For one explosion whose cause is obscure there are ten," says the writer, "which can be traced to causes which an observance of the foregoing rules would assist to remove."

Principes de Science Absolue. Questions de Science Absolue ou Science basée sur une réduction naturelle, intégrale, analogique de l'unité du fait absolu. Par M. JAMES THOMSON. Paris: J. Rothschild.

WE have here a goodly volume, superfine paper, acres of margin, clear bold type, and a singularly tasteful binding. But the contents! We find a string of rhapsodical sentences, printed in a fantastic medley of italics, small capitals, and common characters—not the first time, by the way, that typographical eccentricities have done duty for argument—and lines of asterisks, in the manner of Tristram Shandy, to be filled up at pleasure by the charitable reader, or to be taken as a proof that M. James Thomson, like the parrot in the story, "thinks more than he says." English, or possibly Scotch, as is the author's name, his language is purely French,—not, however, the French of science or philosophy, but of "fast" daily life, interspersed, like boulder-clay, with a new-coined terminology. He aims, evidently, at being profound, original, witty, and aphoristic. He succeeds, to a marvel, in being flippant, grotesque, absurd, sometimes positively loathsome. In support of so heavy a charge, we will quote a passage, after which few readers will care to inquire further:—

"Every individual convicted of *aggressive movements*, atheistic, socialistic, communistic, or anti-social (discourses, writings, conspiracies, rebellions, thefts, pillages, incendiarisms, assassinations, &c. . . . .) is *placed outside the law, degraded* from his *rank as man*, and *assimilated* to the *brutes*.

"This *degradation* and this *assimilation* signify that every individual thus designated is liable, according *to the degree* of his *criminality*, to be condemned to slavery, to torture, to the combats of the circus, whether against other *negativists* or against ferocious beasts; to death; to the shambles.

"It is certain that this last punishment involving the establishment of *shops for selling the flesh of atheists* constitutes a culinary question calculated to chill the souls of gourmets.

"But at any rate a cutlet of atheist is still better than a cutlet of horse, mule, ass, rhinoceros, cat, &c. . . .

"If Paris, when beseiged, had only consented to taste *the atheist*, it might have escaped easily, not alone from the enemies without, but from those within.

"If, in the future, silly *atheophagists* should pretend that MM. the *mimitaptap* of *negation* are not tender, we should advise to pickle them."

Whether this is a stupid jest, or sober (?) sadness, we must pronounce it the most revolting passage which it was ever our misfortune to read. We have here a man original in atrocity at least; capable of surpassing even the Commune and the Inquisition. We have little love or respect, truly, for Robespierre and Cluseret, for Torquemada and St. Dominic. But even they did not eat their victims. "Domini Canes," we suspect, if not too merciful, were at least too judicious.

We find, further, a declaration that the peoples of the true white races "(Germans, English, Americans, French, &c.) are called *naturally*, *necessarily*, *legitimately*, in order to procure the *social elevation* of their own *masses* to *dominate* and to *exploit** the inferior white races (Hindoos, Arabs, &c.), the yellow species, and those of the black species." This is evidently a fundamental idea of "Science Absolue," since it occurs more or less explicitly in several parts of the work. France is repeatedly advised to take possession of China, which would yield her an annual revenue of three or four milliards, whilst Africa would also prove very valuable. All this "domination and exploitation" is indeed to be restricted within the limits of "affirmation," to be marked out, we presume, by M. James Thomson. But whatever theoretical distinctions might be drawn in practice, the result would be simply slavery, and as such it could not fail to prove a curse alike to superior and inferior. The Author holds that the human race consists of three distinct and originally created species, the white, the yellow, and the black, and attempts to prove that this view is in harmony with the Mosaic cosmogony. He informs us that the serpent which tempted Eve was no reptile, but a "heathen Chinee," and that her fall—the eating of the forbidden fruit—was an act of conjugal infidelity of which the result was the birth of Cain.

We suspect that Biblical critics will find his exegetics as little satisfactory as is his ethnology. It is simply inconceivable that the Aryan race should speak of the Chinese as "sons of God," Elohim or Asar, beings superior to themselves. To the Teuton, whom the Author considers as the purest specimens of the "white race," the Asar were his own deified ancestors, ruled over by Odin, king of Gods and men.

M. Thomson informs us that "men are born atheists or communists, just as they may be born rachitic or scrofulous."

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* We have no English word which exactly covers the French "exploiter" as applied to persons. It means to "use up," to "apply to our own purposes," &c.

Yet with admirable consistency he upholds the doctrine of free-will, and repeats the venerable fallacy that those who doubt it have no right to punish a transgressor. How? The wolf and the tiger must of necessity prey either upon our flocks and herds, or upon ourselves. Yet not the less, but rather the more, do we poison or shoot them. If a murderer pleads necessity, society replies that the very same necessity dictates his elimination.

The Author glorifies Adam Smith and Malthus, but rails at J. Stuart Mill, and denounces free trade. Wherever free trade exists, he tells us, "there may be found an Englishman and a dupe." Indeed for England and Englishmen he has little affection. He accuses our country of being the focus of his *bete noire*, the International. He has petroleum on the brain, and is delighted with himself for having turned "proletariate" into "petrolatariate."

But the reader will ask, where is the science? Echo answers "where?" There are, indeed, a few vague generalities, there are positives and negatives, animisms and dynamisms. We are told that the modes of contraction of dynamism are gravitation, weight, cohesion, capillarity, and acoustics, whilst its modes of expansion are heat, electricity, light, and magnetism. We are to ascribe "To the combination of *contractive* DYNAMISM with *infinitesimal* MATTER the apparition of the *germ* of the earth. To the combination of *expansive* DYNAMISM with *contractised* MATTER, the apparition of the *inchoative sketch* of the globe; in a word, the *terrestrial chaos*, organic and inorganic.

To the combination of AFFINITY, or soul of the fourth degree (eumorphism) with a portion of the *dynamised* MATTER, the apparition of the MINERAL kingdom. To the combination of RUDIMENTARY INSTINCT, or soul of the third degree, with a part of the *dynamised* matter, and with *affinity*, the apparition of the vegetable kingdom.

To the combination of TRUE INSTINCT, or soul of the second degree (*animal intelligence*), with a part of the *dynamised* MATTER, with *affinity* and with the rudimentary instinct, the apparition of the ANIMAL kingdom.

Finally, to the combination of INTELLIGENCE, properly so-called, or soul of the first degree (*true soul*, or *reason*) with a part of *dynamised* MATTER with the rudimentary instinct, the true instinct, and with *affinity*, the apparition of the human kingdom."

Such propositions as this can be produced by the mile as easily as calico. But they are not science. They shed no new light on any department of nature; they point the way to no researches, and are as incapable and as unworthy of refutation as of verification. They are essentially cold, sterile, lifeless.

A denunciation of Darwin occurs as a matter of course. The manner of the onslaught is quite in character. M. James Thomson does not argue—he shrieks, chatters, "mops, and mowes" at the great naturalist and his fellow-labourers, in a manner more simian than scientific. But there are no facts which have not been already taken into account; no arguments which have not been repeatedly refuted; no attempts to supersede the doctrine of Evolution by any profounder and wider generalisation, or to account for the phenomena which before its promulgation were unconnected riddles. There is, indeed, a new epithet, "pithecolatry!" It is remarked that if species are not permanent, then the title "Origin of Species" involves a contradiction. How little such an Old Bailey quibble affects the matter at issue is self-evident. To all this Mr. Darwin may well reply, "nolo laudari." M. Thomson is prone to Latin quotations, and can doubtless supply the rest.

It can serve no good purpose to wade any further into this chaos of philosophy, "falsely so-called," of theology, politics, morals, ethnology, and gossip, which the Author has presumed to designate "Absolute Science." Of that "*pourriture*" of the modern world to which he so often refers, this book is certainly not the least offensive symptom.

Fragmentary Papers on Science and Other Subjects. By the late Sir HENRY HOLLAND, Bart. Edited by his son, the Rev. F. J. HOLLAND. London: Longmans and Co.

FOR us, in the last quarter of the nineteenth century, a strange interest attaches to the reminiscences of one who may be said to have stood by the cradle of modern science. "More than sixty years ago," says Sir H. Holland, "Davy showed me, at the Royal Institution, the minute globules of sodium and potassium just obtained from the fixed alkalies. In the same laboratory, the birth-place of so many great discoveries, I witnessed his first experiments on the chemical actions of the voltaic current. Very few years later I heard Dalton expound, for the first time, that Atomic Theory which gave the earliest impulse to those researches of which organic chemistry, present and prospective, is the most wonderful exponent. Yet later, in the theatre of the Royal Institution, I was one of a small party to whom Faraday showed the spark he had just succeeded in drawing from the magnet, the forerunner of those marvellous powers which have since been elicited from the same source." Nor did this early interest in Science forsake the author in his later days. To the last he must have watched with eager interest the progress of discovery, and have kept himself acquainted not merely with the methods and the general results of Science, but even with the minutest details of modern research.

Still it will be felt by the reader that Sir H. Holland writes about Science rather as might a refined and thoughtful scholar than as does the man of Science *pur sang*. His interest lies, after all, in persons rather than in things, physical facts serving him mainly as an introduction to metaphysical reflection. He contemplates rather than speculates. His mind ever reverts to Plato, Aristotle, Lucretius. A striking instance of this tendency may be found in the outset of his Essay entitled "Plurality of Worlds: Are Other Planets Inhabited?" He here informs us that "Neither in the Old or New Testament do we find a distinct answer to the question, though perhaps a few inferential allusions to it. The same may be said of the classical writers: Plato, Aristotle, Lucretius, and Seneca, as far as I can recollect, are silent on the subject. Pliny, who grasps at everything known or imagined, is equally so." This seems to us an instance of what Whewell calls the "commentatorial spirit," so prominent in the Middle Ages, which, instead of enquiring into things themselves, asked rather what former authors had said on the subject.

The most valuable of the Essays before us are, as might be expected, those bearing upon organic nature, especially "Life on the Earth: Relations of Man to Other Animals." Sir Henry here very justly pronounces Bichat's well-known definition of life, "La vie est l'ensemble des fonctions qui résistent a la mort," and that of the "Encyclopédie," "La vie est le contraire de la mort," as "too epigrammatically negative to serve any use." He might have gone further, and pronounced Bichat's utterance one of the greatest absurdities ever uttered by a man of mark. "Life is the sum of the functions which resist death." But what is "death"? The termination of life. If we, then, insert this explanation in Bichat's definition in place of its equivalent "death," we read—"Life is the sum of the functions by which the termination of life is resisted," which is nonsense. Sir H. Holland well points out that these definitions sin by omitting "that which is the very essence of life, viz., that of reproducing life more or less like in kind to itself." But is the search for such definitions scientific? We may ascertain the properties of life, as we do those of electricity or of gravitation. But can we possibly ascertain the essence of any of the three?

We are happy to find that the author has no sympathy with that purblind egotism which would represent man as differing from the lower animals not in degree, but in kind. "The philosopher," he writes, "looking on the dog crouched at his feet, sees in him an animal with organisation akin to his own; with intelligence, memory, feelings, and passions of the same kind, however different in degree and in manner of use; with appetites and necessities of life similar, also, though more in subordination to instincts and hereditary habits of the species. The idle spectator gazes on the anthropoid ape with mere merriment at this mockery of human form and gesture. The man of deeper thought cannot stand in face of these creatures without some feeling of awe in the contemplation of that mysterious scheme of creation which has brought them thus near to himself in the scale of animal being."

The view that the rest of the animal world exists solely in reference to man he rightly treats as a "vulgar notion." Every part of Natural History, and very especially the history disclosed to us by fossil remains, utterly annuls any such conception. It would not be too much to affirm that not one-hundredth part of the animal creation, counted by species, has relation, direct or indirect, to man's existence on earth.

Of the reasoning power of brutes, he remarks—"No happier definition can be given than that of Cuvier: 'Leur intelligence exécute des opérations du même genre.'"

Into a large part of these papers—such as those on Evil in the World, the Perfectibility of Man, Natural Theology, Differences of Religious Belief, Scepticism and Credulity, History, Shakspeare, &c.—we cannot here enter. If the work contains nothing novel in fact or in generalisation, it is, we think, not unsuggestive. The breadth and liberality of its spirit might well atone for even greater deficiency in originality and power.

On a Peculiar Fog seen in Iceland, and on Vesicular Vapour. By R. ANGUS SMITH, Ph.D., F.R.S., &c. London: Taylor and Francis.

METEOROLOGISTS generally suppose that clouds, fog, &c., consist of particles of water in a hollow or vesicular form, something like minute balloons or soap-bubbles. One of the originators of this view was Edmund Halley, the astronomer, who, in the "Philosophical Transactions" (abridged, vol. iii., p. 428), explains the "rising of vapour by warmth, by showing that if an atom of water were expanded into a shell or bubble so as to be ten times as large in diameter as when it was water, such an atom would become specifically lighter than air." It is scarcely needful to remind the reader that water, in order to become specifically lighter than air, would require to be expanded very far more than ten times. In 1743, Gottlieb Kratzenstein, of Halle, in his "Theorie de l'Elévation des Vapeurs et des Exhalations" (Bordeaux, 1743), maintained that "vapours are hollow vesicles," though he admits that they have no absolute lightness and cannot lose their specific gravity. Hamberger, Professor of Physics and Medicine at the University of Zena, also published, in the same year, a dissertation on the subject, to which a prize was adjudged by the Academy of Bordeaux. In this he shows that the weight of water cannot be reduced by increasing the size of the vesicles, or, indeed, by making vesicles at all.

H. D. de Saussure, in his "Essais sur l'Hygrométrie" (p. 282), assumes the existence of vesicles, or hollow spheres, and endeavours to demonstrate their presence in vapour experimentally, and to render them visible.

The attention of Dr. R. A. Smith, from whose pamphlet the above sketch of opinion on this question is substantially taken, was drawn to this subject by a remarkable fog which he observed one afternoon in July, at Reikjavik, Iceland. The peculiarity of the phenomenon consisted in "a larger size of particles than I have ever seen, and that the flatness with which it fell on the ground, and the lumbering mode of rolling, distinguished it from all fogs which I have seen." The people of the town, as well as Dr. Smith and his party, took it at first for dust, or smoke conveying unusually large particles. The wind had nothing to do with the matter, as the fog arose simultaneously from the sea and from a small lake behind the town, and converged in the streets. The particles were found on examination to be at least ten times larger than those described by Saussure. They seemed, moreover, quite solid,—if the expression may be used for a liquid body,—without any hollow centre. This observation accordingly led Dr. Smith to examine the evidence offered by Saussure and others in favour of the vesicular theory. But he found nothing in the least conclusive. The notion of hollow particles "is a vague inference from certain of their qualities, and their supposed analogy to soap-bubbles." The Author contends that the hypothesis of hollow particles, besides being unsupported by facts, is unnecessary, as fine but admittedly solid particles can remain for a very considerable time suspended in the air. As an instance of this kind, he remarks—"Times out of number I have observed, on calm summer evenings, a cloud of smoke from a steam-boat funnel lying for miles in length, at a height very little different from that of the funnel out of which it issued. . . . In these cases have we anything to look to but the size of the particles? They are so small that their resistance to the atmosphere is diminished to the utmost, as the resistance of the air is increased so much in proportion to the weight that they cannot fall rapidly."

The subject is one which demands more attention from physicists and meteorologists than it has hitherto received.

Principles of Metal Mining. By J. H. Collins, F.G.S. London and Glasgow: W. Collins, Sons, and Co.

This little work gives a general and intelligible view of mining operations. One of its greatest merits is that the technical language of the Cornish miners—often a source of no little mistification to geologists, mineralogists, and chemists—is fully explained. The Author insists, very justly, upon the importance of scientific training to practical miners. Referring to instances of valuable minerals being thrown away from ignorance of their nature, he continues—" But it is only the mistakes *which have been discovered* that can be known : it is very probable that the majority of such mistakes are never found out at all. The only safeguard for the future is in the multiplication of mineralogical observers."

Prodromus of the Palæontology of Victoria, or Figures and Descriptions of Victorian Organic Remains. Decade I. By F. McCoy. Melbourne : John Ferres.

This work is the first number of an Appendix to the Geological Survey of Victoria. It is to contain figures and descriptions of the more characteristic fossils of each formation. The present number contains two plates of species of Graptolites, from which the Author determined the Lower Silurian geological age of the slates containing gold reefs. Then follow plates of the extinct fossil wombats from the gold cement of Dunolly, which afforded proof that the Victorian gold-drifts, like those of Russia, are of the age of the mammaliferous crag of the English Pliocene Tertiary age. Next are two plates of volutes, representing the *Volutilites* of the Barton clay formation of Hampshire ; a plate of the *Cycadeous* plants characteristic of the oolitic coal-fields of India, China, Virginia, &c. Finally come plates of characteristic genera of fossil vegetation of the palæozoic coal formation, and of fossil star-fishes from the Upper Silurian rocks.

The work, when complete, will be a most valuable addition to palæontological literature.

A Handbook of Hydrometry. By James Boddey Keene. London : Pitman.

In this little treatise the Author gives an account of the construction and uses of the various instruments employed for taking the specific gravities of liquids. With hydrometers the same confusion prevails as with weights and measures. Almost every country has its distinct scale, and to convert the indications of one of these into another is not always an easy matter. Throughout the civilised world water has, indeed, been adopted as the starting-point, but it is not everywhere taken at the same temperature. " In France water is taken as unity at its greatest density, which is 4·1° C. or 39·4° F. This point has the advantage of presenting no appreciable change of density within a slight range above or below it. It is, however, so far below the average temperature of the year as mostly to require artificial cooling, and to be liable to rapid change of temperature."

In England, with our usual love for inconsistency, we have three standards. The degree ordinarily used is 60° F. = 15·5° C., but the imperial gallon is fixed at 62° F., and the proof point for Sikes's hydrometer, as laid down by Act of Parliament, is 51° F. "These differences," says the Author, "very justly serve no practical purpose, and only tend to confusion."

It is somewhat remarkable that amongst the various hydrometers here enumerated there is no mention of the two most generally employed in England—Twaddell's and the direct specific gravity instrument. Baumé's hydrometers, so much employed on the Continent in chemical works, dye works, &c., are worthy of all condemnation. They are graduated in a very arbitrary and often careless manner, and their indications are not readily calculated into direct specific gravity.

The author has devised a modification of Sikes's hydrometer, ordinarily used by Revenue Officers, which appears to have a decided advantage.

Consumption and Tuberculosis, their Proximate Cause and Specific Treatment by the Hypophosphites. By J. F. CHURCHILL, M.D. London: Longmans and Co.

MEDICINE was reproached by the late Mr. Buckle as being still in the "theological stage." "Not one of its so-called specifics," continues the historian of civilisation, "has been discovered deductively, or even justified *a priori*." The Author holds that in the hypophosphites we have a specific for "pulmonary affections arrived at *a priori* by hypothetic induction, verified by clinical experiment, and confirmed by a concordance of the widest and most numerous array of facts ever brought forward in pathology."

Dr. Churchill was led, by his observations and experiments, to assume—provisionally, at least—the two following propositions:—

"The tubercular diathesis is owing to a diminution in the system of the phosphorus element."

"As this element fulfils the function of a combustible body, it must be in a degree of oxidation inferior to that of phosphoric acid."

He then proceeded to submit his theory to practical verification. Free phosphorus being, from obvious reasons, inadmissible, the choice lay between the oxide of phosphorus and the phosphorous and hypophosphorous acids. He selected the latter in the state of a lime-salt, and by experiments upon himself established the fact that it could be taken in 6-grain doses without any ill effects. He then proceeded to actual practice. Cases of consumption are, unfortunately, sufficiently common, and the results of the ordinary treatment are so little satisfactory, that the physician is certainly morally justified in adopting any new system which holds out any rational probability of success. A number of cases, with their results, are described. Not being ourselves connected with the Faculty we cannot, of course, view this matter professionally. But if we are asked whether we—judging from the "standpoint" of the man of science—consider that Dr. Churchill's facts support his theory, we reply without hesitation in the affirmative. Of course we do not admit, nor does the author contend, that hypophosphites, no matter of what base, no matter in what dose, or in what admixture, will inevitably cure phthisis, irrespective of the stage of the complaint and of possible complications. Such a proposition could only be upheld by the charlatan or the routinist, as distinguished from that too rare character—the thoroughly scientific physician. The following passage will show that Dr. Churchill by no means lays claim to such infallibility:—"What science or what art but that of medicine has ever pretended to an 'infallible' process? 'Infallible' means unconditional. Art, on the contrary, is the attainment of definite ends by definite means under certain given conditions, and the object of all science is the discovery of these means and of the conditions which are to guide us in their use."

There are few scientific men, in the true sense of the word, who will be able to read this book without pleasure and profit, and without recognising the profundity, the acuteness, and the originality of the author.

Reverting to the Preface we find a mournful, and only too true, picture of the difficulty experienced in obtaining fair play for a novel idea:—

"To whom could such an appeal be made?

"To the statesman?

"He will point a speech with a sanitary conundrum, and think no more of the matter.

"To the health-reformer?

"He is all eagerness that the people should be dragooned into health, even as their forefathers were once dragooned into holiness, and he hopes to be one of the dragooners: beyond that he goes not.

"To the man trained in the severe and patient methods of positive science?

"He will be afraid to venture amidst the discordant facts, the rash assumptions, and the ever-shifting fallacies of the so-called science of therapeutics.

"To the philanthropist, our poor clipt and crest-fallen substitute for the old knight-errant?

"Even he will fail to see that there is here the means of doing more good than by the endowment of many charities."

The Author quotes, from a medical authority whom he does not name, the theory that "pulmonary consumption is a bountiful dispensation of Providence to eliminate the weak and imperfect, and thus purify the earth, in order to save us from becoming a race of pigmies, of sickly dwarfs, and eventually dying out." It is distressing, and at the same time incomprehensible, how any man who has enjoyed any rudiments even of a scientific training can give vent to such an unscientific outbreak of gratitude. Pulmonary consumption prevails mostly during the *prime of life*, and numbers of its victims have consequently, before they are carried off, become parents! If it were the plan of Providence "to eliminate the weak and imperfect," the disease, surely, ought to occur in early childhood; or, better still, the first symptom of debility in any organism would be the cessation, temporary or permanent, of the reproductive power. But we often find that sickly beings, both animal and vegetable, are more prolific than the healthy. Shall we never be able to free ourselves from the delusions of teleology?

We can only hope that our voice may be, at any rate, of some little service in securing for Dr. Churchill that candid hearing which is all he demands.

Text-Book of Botany, Morphological and Physiological. By JULIUS SACHS, Professor of Botany in the University of Würzburg. Translated and Annotated by A. W. BENNETT and W. T. THISTLETON DYER. London: Macmillan and Co.

IT has been insinuated, somewhat wickedly, that since the disuse of the Linnæan system Botany has become much less popular among ladies than it was heretofore. The change, indeed, is complete. We can imagine a botanist of the old school whose highest ambition it was to collect specimens from our scanty British flora, and to ascertain their names according to an artificial system, feeling bewildered at a work such as the one before us. He would find classification no longer the be-all and end-all of plant-lore, developed rather in its fundamental principles than in the minutiæ of detail. He would find species grouped together, not with regard to one single set of organs, but in accordance with their entire structure and the laws of their evolution. Had he the patience and perseverance to study the volume, he would be bound to confess that the botany of the present, if making larger demands upon the faculties of his votaries than did the botany—we might say the herbalism—of the past, it rewards their exertions in a far more bountiful manner.

The work of Professor Sachs is so thorough-going, so complete, and presents the results of the most recent investigators in such a clear and intelligible manner, that criticism becomes almost superfluous. We have certainly no English work which can take its place as a manual of vegetable morphology and physiology. The translators well deserve the thanks of all friends of the organic sciences for having placed this rich treasure within the reach of the English student.

The chapters on the elementary constituents of the food of plants; on assimilation and metastasis (*stoffwechsel*); on the respiration of plants; and on the action of temperature, light, electricity, and gravitation upon vegetation, are most excellent, and may be consulted with advantage by the physicist and the chemist, as well as by the botanist.

The last chapter is devoted to that difficult but fascinating topic, the origin of species. It may be interesting to learn what are the views of so eminent an investigator as Prof. Sachs upon this crucial question. He declares that—"The scientific basis for the theory of descent rests in the fact that it alone is able to explain in a simple manner all the mutual relationships of plants to one another, to the animal kingdom, and to the facts of geology and palæontology, their distribution at different times over the surface of the earth, &c.; since it requires no other hypothesis than descent with variation and the continued struggle for existence which permits those forms only to persist that are endowed with sufficiently useful properties, the others perishing sooner or later. But both these hypotheses are supported by an infinite number of facts. The theory of descent involves only one hypothesis which is not directly demonstrated by facts, namely, that the amount of variation may increase to any given extent in a sufficiently long time. But since the theory which involves this hypothesis is sufficient to explain the facts of morphology and adaptation, and since these are explained by no other scientific theory, we are justified in making this assumption."

The Transit of Venus. By GEORGE FORBES, B.A. ("Nature" Series.) London and New York: Macmillan and Co.

THE work before us is based upon a paper originally read before the Philosophical Society of Glasgow, and subsequently published in "Nature." There is scarcely any point connected with the entire question but receives a lucid explanation. The reader will learn what is a transit of Venus, the reason of its rarity, and its value in enabling us to calculate the true distance of the earth from the sun. We also find an explanation of the term "parallax,"—not, of course, of the gentleman who assumes that name,—and a brief notice of other methods by which the sun's distance may be determined, such as the parallax of Mars when nearest the earth, which occurs every fifteen years; the method founded on the velocity of light and the procedure of Leverrier, materials for which are gradually accumulating. The

Author then gives an account of the various methods in which a transit of Venus may be utilised, namely, the determination of the times of contact, at different stations, of known longitudes, and the determination of the least distance between the centres of the sun and Venus during the transit, as observed from different stations. This last determination may be conducted either photographically, heliometrically, or by the method of durations. These methods and the needful apparatus are described at some length. We cannot help referring to the photographic method as a beautiful example of the inter-dependence of the sciences, and how one may furnish methods to another. Who would have dreamed, half a century ago, that chemistry would—in photography and spectroscopy—furnish the astronomer with two novel and valuable means of research? The book concludes with an account of the stations selected by various nations, and an enumeration of the English observers.

To every one who, without making astronomy his speciality, wishes to have a full and clear knowledge of this subject in all its bearings, Prof. Forbes's book is indispensable.

The Province of Psychology. The Inaugural Address at the First Meeting (April 14, 1875) of the Psychological Society of Great Britain. By the President, Mr. Serjeant Cox. London: Longmans and Co.

THIS Address is, in the fullest sense of the word, "inaugural." Its object throughout is to define the work of the Society, and this object is admirably carried out. In the Preface we are told that the Psychological Society embraces no creed, supports no faith, contemplates no theory, has no latent designs, but proposes only to collect facts and investigate psychological phenomena, precisely as other scientific societies investigate the phenomena of their respective branches of knowledge; and, further, that a very erroneous notion respecting its objects appears to prevail, viz., that it has been established with a special view to the promotion of a new faith, to which the name of "Spiritualism" has been given. This notion is vigorously and authoritatively contradicted by Serjeant Cox, and the undeviating tone of the Address throughout is in perfect harmony with such contradiction. If Spiritualism be a delusion, and if the Psychological Society rigidly and vigorously follow the course which Serjeant Cox has mapped out for it, this Society will assuredly do more than has ever been done hitherto towards the removal of the delusion; if, on the other hand, the error is on the other side, its refutation will ultimately be effected by the same means.

The function of the Society, as defined in this Address, is to apply to all questions connected with "*Life, Mind, and Soul*" the strictest methods of inductive investigation. In reference to the evidence demanded, Serjeant Cox says—"It is an inflexible rule of our Courts of Law that the best evidence only shall be accepted, and that secondary evidence shall not be received when primary evidence can be had. It is a rule of reason and common sense. Its observance is no less essential to scientific investigation, and I trust that by this Society no relaxation of it will be permitted. Necessarily we shall be called upon to deal with some reports of alleged phenomena of rare occurrence and transcending common experience. It is scarcely necessary to remind the members that a higher degree of proof should be required in proportion to the strangeness of the phenomenon, and that strictest scrutiny must be made into the minutest details before the Society will be justified in giving to it a place among its records of psychological *facts*."

Theology is to be excluded from the deliberations of the Society. The desirability of this is unquestionable. The subjects of investigation are precisely those which, without such exclusion, would inevitably drift into sectarianism, and open the portals of superstition. At the same time, it is equally obvious that the difficulty of keeping clear of theology will be considerable, but this difficulty must be boldly faced, as the danger and mischief are precisely proportionate to the proximity of the subjects. Serjeant Cox treats this part of the subject but lightly; and we think that a clear definition of the boundaries between psychology and theology is much needed, and we venture to suggest the following for the serious consideration of the Society:—*Psychology is a science of experiment and direct observation, while neither direct observation nor experiment are possible in theology*. *Indirectly* the Society may do good service in promoting sound and wholesome theology; for although the Society, as a Society, will deal only with the facts and laws of the operations of human life, mind, and the "psyche,"—as Serjeant Cox prefers to name the possibly separate essence,—and thus will deal with matters of observation and experiment, the individual member, each for himself and on his own responsibility, may make such theological inferences as present themselves to his own mind. The separation of these subjects in the Proceedings of the Society does not necessarily imply any separation of them in the thoughts of the individual members, and still less does it suggest any exclusion of theology or religious sentiment.

Many special subjects of investigation are suggested in this Address,—such as the great question of matter and non-matter; whether our consciousness, our individuality, ourselves in fact, are merely cerebral functions beginning and ending with cerebral action, or whether a separate something or "psyche" exists which uses the brain and bodily mechanism as the organ of its manifestation. Whether the brain acts as a whole, or is composed of separately acting parts, as affirmed by Gall. Whether each hemisphere of the brain is a distinct organ conferring duality of mind, as also stated by Gall and re-affirmed by Brown-Sequard. What is matter? "Is matter merely the incrustation of spirit-atomic structure aggregated into molecular structure on the surface, as it were, and passing continually from one to the other—as the atmosphere becomes visible in the form of a cloud when it comes in contact with a colder body? Or is it that the vast interspaces between the worlds, those regions void to our senses, in which those countless worlds are as grains of dust, are really thronged with life which—because it is not of molecular structure—is imperceptible to our very limited material senses?"

Heredity and Hybridity are especially mentioned, and all those phases of the doctrine of Evolution which relate to the Descent of Man. Sleep, Dreams, Somnambulism (Natural and Artificial), Delirium, Insanity, and the Influence of the Mind upon the Body, are also among the subjects named and partly discussed in this Address.

We dare not go further than the mere enumeration of these, as any discussion of either would lead us far beyond the limits of this notice, and must conclude by heartily wishing that the Psychological Society will strictly adhere to the programme and principles prescribed by Serjeant Cox. If it does so, it cannot fail to do good service to the highest of all the sciences.

Principles of Mental Physiology; with their Applications to the Training and Discipline of the Mind and the Study of its Morbid Conditions. By WILLIAM B. CARPENTER, M.D., LL.D., F.R.S., F.L.S., F.G.S., &c. London: H. S. King and Co.

THIS work is described by its Author as "An Expansion of the Outline of Psychology, contained in the fourth and fifth editions of my 'Principles of Human Physiology'" (1852 and 1855). The keynote of the whole work is struck in the next paragraph of the Preface, where Dr. Carpenter tells us that "Not having seen reason to make any important change in my own psychological views since I first put them forward, but, on the contrary, having found them confirmed and extended by the experience and reflection of twenty years, I set myself to revise my former exposition of them." The present work is an expanded result of this effort. Readers who are desirous of admiring the immobility of Dr. Carpenter's "psychological views," and of studying the state of psychology at the period named, will find what they require somewhat irregularly diffused through the 708 pages of this work.

Dr. Carpenter's psychological immobility, his faithful adherence to the lessons of his youth, are well shown in Chapters i. and ix., which treat of the "General Relations of the Mind and Body," and of "The Will" The physiology and philosophy of evolution is ignored throughout, and the doctrine that the human will is a something which exists outside and independent of the cerebral functions, is laboriously advocated. Dr. Carpenter tells his readers that "the actions of our Minds *in so far as they are carried on without any interference from our Will, may* be considered as "Functions of the Brain." On the other hand, in the control which the Will can exert over the *direction* of our thoughts, and over the *motive force* exerted by the feelings, we have the evidence of a new and independent Power, which may either oppose or concur with the automatic tendencies, and which accordingly as it is habitually exerted, tends to render the Ego *a free agent.* And truly, in the existence of this Power, which is capable of thus regulating the very highest of those operations that are casually related to corporeal states, we find a better evidence than we gain from the study of any other part of our Psychical nature, that there *is* an entity wherein Man's nobility essentially consists, which does not depend for its existence on any play of Physical or Vital forces, but which makes these forces subservient to its determinations." This passage—the capitals and italics of which are Dr. Carpenter's—summarises pretty fairly the "view which it has been the special purpose of this Treatise to develop." It would carry us far beyond the space we can devote to this book to point out a tithe of the inconsistencies and contradictions which necessarily result from an attempt to reconcile with established physiological laws this notion of an outside will overruling cerebral function. The mere metaphysician, reasoning only on the data furnished by an examination of his own consciousness, might plausibly enough maintain such a doctrine; we might expect it from a certain school of theologians, but that a physiologist, and one who comes forward as a public teacher of this branch of science, should in 1874 publish the two chapters above referred to, is in itself a psychological anomaly sufficiently curious to be worthy of a place among the cases of aberration which Dr. Carpenter has quoted in the course of Chapters xiii. to xviii.

In these Dr. Carpenter treats of Unconscious Cerebration, Electro Biology, Sleep, Dreaming, Sonnambulism, Mesmerism, Spiritualism, Intoxication, Delirium, and Insanity. He is evidently more at home here than in the previous chapters on Memory, Common Sense, and Imagination, and hence Chapters xiii. to xviii. form the most interesting part of the volume, the illustrative cases being very numerous, and for the most part appropriate.

As a matter of course "Unconscious Cerebration" receives a large share of attention, and is made to explain a vast deal.

Dr. Carpenter's "unconscious cerebration" is evidently but another name for what Helmholtz has termed "unconscious judgment." We see no advantage in the more pedantic paraphrase which will not bear critical examination. We may arrive at a conclusion either in perception or reasoning by a sound, healthful, and conscious judgment, *i.e.*, by an intellectual effort, each step of which is consciously understood, and therefore capable of critical re-examination; or we may do the same by means which are hidden to ourselves, and which therefore we cannot thus critically examine. In the latter case the conclusion or judgment may be a delusion to the true nature of which our unconsciousness of its origin renders us blind. Such unconscious judgment would thus be an abnormal or morbid judgment. We therefore see no objection to this term of Helmholtz's, provided its application be not overstrained. But Dr. Carpenter's "unconscious cerebration," as applied to abnormal or morbid action, is a physiological contradiction, inasmuch as *cerebration, when normal and healthy, is always an unconscious operation.* A man with a healthy brain is conscious of seeing, hearing, feeling, judging, thinking, &c., but is not conscious of *cerebrating*. It is only when the brain is diseased or overworked that we are conscious of any cerebration whatever, and even then the consciousness is very indefinite. If the term unconscious cerebration is to be used at all, it should be applied to healthy action of the brain, while *conscious cerebration* would describe certain peculiar states of its morbid action.

On the whole this work of Dr. Carpenter's is by no means a satisfactory effort. Had it been published twenty years ago it might have had some value as a text-book, but a thick book on "Mental Physiology," that almost totally ignores the important contributions that have been made during the past quarter of a century by Helmholtz, Huxley, Herbert Spencer, and other leading philosophical physiologists, is of little use to anybody. It is true that Dr. Ferrier's recent investigations are described in an appendix, but the student who only takes Dr. Carpenter's book for his guide will naturally suppose that the method of investigation adopted by Dr. Ferrier was something new and original.

The methods of experiment, and the main results described in this Appendix as Dr. Ferrier's, are really those of Fritsch and Hitzig, Ferrier having merely followed up and confirmed the investigations made three years previously by these German physiologists. Dr. Hitzig and some of his compatriots have loudly complained of the unjust treatment which he and Fritsch have received in England, and these complaints appear to be mainly founded upon this ignoring of their work and existence by Dr. Carpenter when describing their methods and results. The German physiologists appear to have taken it for granted that Dr. Carpenter's "Mental Physiology" is one of our accepted text-books, and that it represents the English estimate of their work. The disclaimers that have been already made on the part of English physiologists have doubtless ere this convinced Dr. Hitzig and his friends that such is not the case.

Elements of Practical Hydraulics, for the Use of Students in Engineering and Architecture. Third Edition. Part I. By Samuel Downing, LL.D. London: Longmans and Co.

The constantly extending use of water, and of machinery and appliances in connection therewith, renders it very desirable that the engineering student should be well versed in all branches of hydraulics, and every additional facility for acquiring a correct knowledge of the principles upon which that science is founded is well deserving of acknowledgment. The present work—which has now reached its third edition—is one of an eminently high character, and having undergone a careful revision prior to publication, is—what such a book ought to be—a very complete and valuable contribution to scientific literature, and is no mere science text-book compiled exclusively from other works, and edited by one having little or no knowledge of the subject treated on. Mr. Samuel Downing's name is well known as that of a careful author, whilst the manner in which the work now under review is arranged and compiled shows clearly that that gentleman thoroughly understands the subject on which he writes. This volume does little more than introduce us to the first principles of hydraulics; it consists of but three chapters, which treat respectively on the discharge of water through an orifice, and over weirs, waste-boards, or overfalls; on the flow of water under variable heads, with examples and practical applications as applied to sluices and weirs; and on the flow of water through pipes, channels, and rivers. Under the above headings we are introduced to the first principles of hydraulics as applicable to irrigation, river improvement, and water supply, the whole subject being thoroughly worked out; and, whilst the general explanations contained in the text are clearly and intelligibly given, very numerous formulæ are furnished, together with tables of co-efficients, as ascertained by different observers under varying circumstances. There are fewer works on hydraulics amongst English scientific literature than could be desired, but many are to be found of great value by French writers, and from these Mr. Downing has freely drawn, giving the results of experiments therein recorded, thus enriching the store of knowledge on this important subject with which his book abounds.

Elements of Practical Construction for the Use of Students in Engineering and Architecture. Part I. By SAMUEL DOWNING, LL.D. London: Longmans and Co.

THIS is another work by the same author as the above, and is noticeable on account of the great care and precision with which it has been compiled. In this volume the resistance of materials to direct compression and tension only is treated, leaving to another volume the subjects of elasticity, indirect compression and tension, transverse resistance and torsion, &c. It must be clear to every one interested in the subject of construction how very important it is that the real strength of materials employed should be perfectly understood, and that they should know how to calculate the factor of safety under varying circumstances. This is more difficult when varying or transverse strains have to be allowed for, but should be comparatively easy in the case of direct compression and tension only. These latter may generally be classified under four heads, viz.—First, the resistance to direct extension; a force being in action tending to tear asunder the fibres or particles of the body, as is the case in tie-rods, the links of the main chains of a suspension bridge, chain cables, screw-bolts, &c. Second, the resistance to direct compression; a force being present tending to crush the particles or fibres, as in pillars, piers of bridges, shafts of columns, &c. Third, the resistance of a beam supported at one or both extremities to a force acting in a direction transverse to its length, and in fibrous material, as wrought-iron and timber, perpendicular to the line of fibres, which force is illustrated in the girders of bridges, beams, &c. Fourth, the resistance to forces of torsion which act so as to twist the fibres, as in crank-axles, the shafting of machinery, capstans, &c. Besides the above, all of which are clearly dealt with in the present volume, many other complicated varieties in the mode of action of the resisting powers of bodies may be enumerated, such as that which takes place from the interior to the exterior surface of hydraulic presses, and of pieces of ordnance when fired, and so forth, to which some reference also is made in Mr. Downing's book. The principle upon which this work is based is that first giving a proposition for treating each material, stating its average ultimate resistance: this is followed by experimental proofs, and then are given illustrations of the material so strained, taken from completed and successful structures of eminent engineers.

The publication is a very valuable one, and seems especially adapted for the purpose for which it is primarily intended, namely, for the use of students, to whom we can confidently commend it for study, whilst it will, no doubt, also prove valuable for reference by those engaged in practical work.

CORRESPONDENCE.

THE POLE STAR AND THE POINTERS.

By Capt. ALLAN CUNNINGHAM, R.E., Hon. Fell. of King's Coll., Lond.

SIR,—Attention has been drawn (in No. xliii. of the "Quarterly Journal of Science) to an interesting phenomenon connected with the well-known rule for finding the Pole Star by means of the "Pointers" (α and β Ursæ Majoris). It is there asserted that it is matter of popular observation that the "Pointers" *do not at all times appear to "point" equally well* to the Pole Star, and an attempt is there made to prove that popular observation is herein *correct in fact*, that is to say that the "Pointers" *do not actually at all times "point" equally well* to the Pole Star.

The present writer, however, considers that the true explanation of the phenomenon has been quite missed in the Article referred to, and that the Article itself, indeed, contains its own refutation.

The true explanation to the present writer's mind is that—

1. The Pointers *do not actually* at all times (of the same night) "point" equally well to the Polar Star by an amount depending solely on aërial effects of refraction.
2. The Pointers *do not appear*—to the unaided human eye—to "point" equally well at all times (of the same night) to the Pole Star by an amount depending solely on an error of judgment (of the observer) in tracing the "directing line" in the heavens, and which *apparent change* of the obliquity of the "directing line" must therefore be considered an OPTICAL ILLUSION.
3. There is a minute annual change of the "directing line"—due to the earth's orbital motion—appreciable only with difficulty by the best instruments.
4. There is a slow secular change in the relative positions of the three stars—due to their proper motions—in consequence of which the "directing line" of the Pointers will in the course of ages cease to "point" towards or near the present Pole Star, but this change will be *very slow*.

The first two effects of *actual* and *apparent* change (during the same night) of the obliquity of the "directing line" of the Pointers are the only ones of present interest.

It may be premised that both effects will be zero at the North Pole, and extremely small in high northern latitudes; and will both increase with proximity to the Equator, the latter increasing much the more rapidly.

The former effect — the actual change—would be so small in Great Britain as to be appreciable only with instruments, but would probably become appreciable to the unaided eye in low northern latitudes: the latter effect—the apparent change (merely an optical illusion)—will rapidly increase with distance from the North Pole, and is perceptible in Great Britain.

As both effects increase with proximity to the Equator, the joint effect would of course be more noticeable in low northern latitudes, especially to one leaving a considerably higher northern latitude; indeed the attention of the writer of the previous Article seems to have been first attracted (see p. 286 of No. xliii. of the "Quarterly Journal of Science") to the phenomenon when on a voyage *towards* the Equator.

That the principal part of the apparent change of obliquity of the "directive line" of the Pointers from the Pole Star is really an error of judgment—*i.e.* only an *apparent* change, or an optical illusion—may be with some trouble *experimentally* verified by any one who has a small *portable* equatoriably mounted telescope (a "comet-seeker" would do very well)—it would be better if fitted with clockwork motion—or a small transit theodolite. The experiment would probably be more convincing to many than any amount of purely geometrical reasoning as to what *ought to be*. It will be described as performed with a small equatorial fitted with a set of hairs in its diaphragm, and means of illuminating the hairs.

Experiment.—The instrument must be set up so that the centre of the middle hair shall—by moving the telescope round the declination axis only—traverse both Pointers: the proper position will differ only slightly from the usual position of an equatorial, and may be found—by trial—by setting up the instrument as usual, *i.e.* with the polar axis pointing to the North Pole, and then correcting the position *by shifting the polar axis*, partly in azimuth, partly in altitude. The correct position, *viz.* that in which the centre of the middle hair traverses both Pointers *on rapidly moving the telescope round its declination axis*, will be secured after a few trials. When this position has been secured, the reading of the hour-circle should be noted: after which, turn the telescope *round its declination axis*, as near the Pole Star as possible—when, if the telescope be of low power, the Pole Star will be in the field of view: the centre of the middle hair must now be made to cover the Pole Star by use of the motions *round both polar and declination axes*; after which the hour-circle must be read again. One observation is now complete: the difference between the two readings on the hour-circle will be the *deviation* ($p\ m$ in Fig. 1) of the "directive line" of the Pointers from the Pole Star—affected, however, to some extent by the rotation of the earth during the interval of observation.

It is very desirable that the whole observation—from the moment the instrument is in proper position—should be rapidly performed, so as to reduce the last effect as much as possible: the apparent motion of the Pole Star is so slow, that, if the operation be rapidly performed, the earth's rotation will not sensibly affect the observation.

The above process should be repeated at other times of night: the *position* of the instrument will require slight shifting for every observation to secure the necessary condition that the centre of the middle hair should traverse both Pointers by the rapid motion of the telescope round the declination axis.

It is obvious that a transit theodolite might (with some trouble) be used in the same manner by fitting it with a strong universal joint and clamp, so that it might be possible to tilt its horizontal plates into the same position as the hour circle of the equatorial. Care must be taken that the joint and clamp are strong enough and stiff enough to hold the whole weight of the instrument steady throughout the observation.

It will be found that the values of the "deviation" of the "directive line" of the Pointers obtained from different observations will—if the observations have been well made—not differ from each other by any such large amount as would be appreciable by the unaided eye.

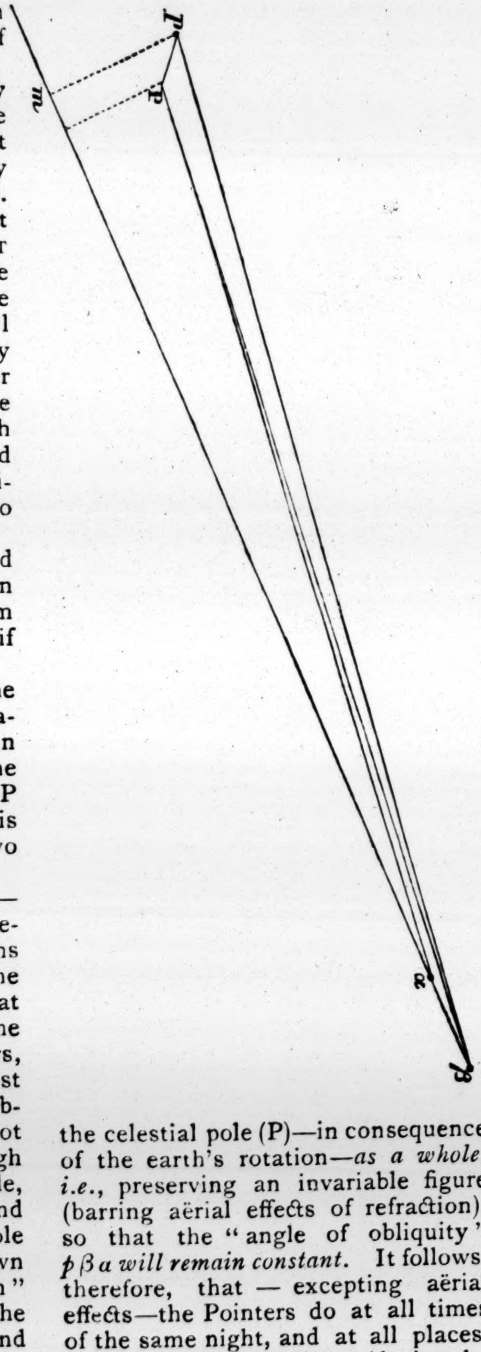
The result of this experiment is a proof positive that the principal part of the apparent change of obliquity of the "directive line" is only an *apparent change*, *i.e.*, an optical illusion.

The small difference in the values of the "deviation" *as obtained from different observations*, are due to the following causes:—1. An *actual change* due to aërial effects of refraction. 2. Errors of observation due partly to errors of the observer, and partly to the imperfection of the experiment in introducing improperly effects of the earth's rotation.

It will now be shown that the above result might have been expected *à priori*, *i.e.*, that—barring effects of refraction—there is no change in the obliquity of the "directive line" of the Pointers from the Pole Star at different times or places.

Consider first the meaning of the word "point" as here used of stars. The popular explanation would probably be—

Popular Explanation.—"Two stars are said to 'point' at



a third when an apparently straight line joining them passes through the third, if produced."

Now this definition is only tolerably good for *very small portions* of the celestial sphere: apparently straight lines cannot be drawn at all over any considerable portion of the sphere. The only real analogue to straight lines in plane is that of GREAT CIRCLES on the sphere, which are really the *shortest lines* which can be drawn between two points: small portions of Great Circles do really seem *apparently straight*: no other lines on the sphere can possibly be apparently straight, but are both *actually* and *apparently curved*, and in general *twisted*. The proper definition of "pointing" as applied to stars is therefore—

Definition.—"Two stars are said to 'point' at a third, when a Great Circle through them passes through the third if produced."

Apply this to the case of the Pointers and Pole Star. Their relative positions are *plotted to scale* in Fig. 1 from data taken from the "Nautical Almanac." In the figure P is the north pole of the heavens, p is the Pole Star, α and β are the two Pointers (α and β Ursæ Majoris).

The lines Pp, Pβ, Pα, $\beta\alpha$, βp—straight lines in the diagram—represent the Great Circles in the heavens joining the points named: thus the (straight) line $\beta\alpha$ represents the Great Circle in the heavens, which is the true "directive line" of the Pointers, and along which, therefore, they must be said to "point." It will be observed that the Pointers (α, β) do not lie on the same Great Circle through P (the Great Circles Pα, Pβ, include, in fact, an angle of about 30′) and therefore do not "point" to the pole (P); the Great Circle arc $p\,m$, drawn perpendicular to $\beta\alpha m$, is "deviation" of the "directive line" $\beta\alpha m$ of the Pointers from the Pole Star (p), and subtends at the further Pointer the angle $p\beta\alpha$, which is what has been styled above the "*angle of obliquity of the directive line*" of the Pointers from the Pole Star.

Imagine this figure traced in visible glittering lines in the heavens at any instant: it will seem to swing round the celestial pole (P)—in consequence of the earth's rotation—*as a whole*, *i.e.*, preserving an invariable figure (barring aërial effects of refraction), so that the "angle of obliquity" $p\beta\alpha$ *will remain constant.* It follows, therefore, that — excepting aërial effects—the Pointers do at all times of the same night, and at all places, "point" equally well to—(deviate by a constant quantity from)—the Pole Star.

It is easy to verify the above by a simple experiment:—

Experiment.—Take an orange (or a worsted ball) to represent the celestial sphere: drive a knitting needle through it,—this will be a tangible representative of the axis of apparent diurnal rotation of the heavens,—but only far enough to let one end just project; this end will represent the North Pole (P) of the heavens. Next drive three pins up to their heads into the orange in positions similar to p, β, a in the figure: the three pin-heads will represent the Pole Star (p), and the two Pointers (a, β). Strain a fine thread from the pin p round the pins β, a, and pin it down again at m, a point in the direction of βa produced. The thread (if properly strained) will cling to the orange along the Great Circles $p\beta$, $\beta a m$. The construction is now complete.

Face the north, and hold the orange with the projecting end of the needle pointing towards the North Pole of the heavens (which it represents), and turn the orange slowly round the needle (held in the hand) in the same direction as the stars appear to move, *i.e.* rising from the east or right hand: the set of pin-heads representing the Pole Star and Pointers will now move in precisely the same manner as the apparent diurnal motion of the actual Pole Star and Pointers, and it will be at once recognised that the "angle of obliquity ($p\beta a$) of the directive line" continues *invariable*, *i.e.*, that the Pointers do always "point" equally well to, *i.e.* with equal deviation from, the Pole Star.

It will also be understood that the Great Circle ($\beta a m$) through the Pointers was the line traced out in the heavens by the centre of the middle hair of the equatorial in the first experiment, and that the arc $p m$ was the angle obtained as the difference of the readings on the hour-circle.

Effects of Aërial Refraction.—The effect of the earth's atmosphere on the rays of light by which the stars become visible to us is to raise the apparent position of all by a small amount, so that all *appear higher* than they would if no atmosphere existed. The amount of apparent increase of altitude depends on the actual altitude, and is greatest (about thirty-three minutes) for a star in the horizon, and rapidly diminishes with increase of actual altitude, and vanishes at the zenith.

Now as the Pole Star and Pointers are generally at very different altitudes above the horizon, the apparent increase of altitude is generally different for each of the three, being always greatest for the lowest, and least for the highest; moreover this apparent increase of altitude changes in magnitude slowly from instant to instant, the variation being different for each of the three, *viz.*, least for the Pole Star, which can never be more than about 1½° above or below the pole, and greatest for the further Pointer (β), which may be about 32° 49′ above or below the pole.

The effect of this varying increase of altitude *of the visible positions* of the three stars (different in amount for each star) on the problem in hand is a slight change both of *position* and of *shape* of the spherical triangle $p\beta m$ from the normal form (of the diagram) which it would have (excluding atmospheric effects), and this change varies slowly from instant to instant. Thus the *apparent shape* of the spherical triangle $p\beta m$ does really vary throughout the night, and therefore also the "apparent deviation" ($p m$), and also the "apparent angle of obliquity" of the Pole Star from the Pointers *do really vary throughout the night.*

This effect is, however, zero at the North Pole of the earth, for all three stars will there be *apparently* raised by a constant small amount (different, of course, for each star, but invariable for the same star): thus the *apparent shape* of the spherical triangle $p\beta m$ will be (slightly different indeed from its true shape, aërial effects excluded, but) of *invariable* form throughout the night, so that the "angle of obliquity of the directive line" of the Pointers is both actually and apparently invariable at the North Pole of the earth.

The effect due to refraction will increase at first very slowly from the North Pole towards the Equator, and will be sensibly zero in all high northern latitudes, and even in Great Britain will be so small as to be inappreciable to the unaided eye. The effect will increase more rapidly towards the Equator. The solution of the following Problem—

Problem—"In what latitude, and at what hour does the effect of aërial refraction cause most variation in the 'deviation' of the 'directive line' of the Pointers from the Pole Star (apparent positions only considered), and what will that maximum variation be?"

would be a matter of considerable mathematical, or rather numerical, complexity, and is hardly worth the great labour it would probably involve. It seems, however, *probable* that this maximum would occur at or near the Equator; and it is *possible* that it might be large enough to be appreciable by the unaided eye.

Explanation of Cause of Optical Illusion.—It having been shown that the principal part of the apparent change during the same night of the "angle of obliquity of the directive line" of the Pointers is *only an apparent change*, having no existence in fact, and which can therefore only be regarded as an OPTICAL ILLUSION, it remains to endeavour to give some explanation of the cause of so extraordinary an illusion.

It has been explained that the true meaning of the phrase "two stars *point* at a third" is that the Great Circle through the two stars passes—if produced—through the third. Unless, therefore, the eye can properly trace out and produce a Great Circle in the heavens, the mind cannot fairly judge of the manner in which two stars "point"—whether well or ill—at a third.

The illusion appears to depend entirely on the fact that the tracing of Great Circles in the heavens is an unfamiliar matter. This may readily be tested by trial. Let anyone endeavour to trace Great Circles in the heavens between pairs of stars not very far apart, and produce them as far as possible in both directions, and then compare them with the Great Circles obtained by straining a fine string through the same pairs of stars on a celestial globe; he will be astonished at the discrepancy when the Great Circles are not vertical or nearly so. Let it be remembered that *every* such Great Circle *ought to cut the horizon* in two points diametrically opposite. There will be found no difficulty in tracing out vertical or nearly vertical Great Circles tolerably correctly; but whenever the true Great Circle would be considerably inclined to a vertical circle, it will be found that—if the stars be not very far apart—the Great Circle as traced by the unaided eye will not cut the horizon in two points diametrically opposite, nay—if the obliquity be very great—will not cut the horizon at all!

The eye seems to be influenced in some way by an impression of the position of the horizon; so that if pairs of stars be selected at the same or nearly the same altitude, the impulse to draw a *horizontal circle*—which is, of course, a small circle of the sphere—through them will be found almost irresistible!

Anyone who tries this experiment fairly a few times will soon be convinced that the *apparent change* of "deviation" of the "directive line" of the Pointers from the Pole Star exists only in imagination, and is a remarkable instance of optical illusion.

The cause of the great influence of the horizon on the attempt to trace oblique Great Circles in the heavens is probably that all our associations of practical life are with figures drawn on the earth, *i.e.* on a horizontal plane, and that the recollection in the mind of these, the familiar work, insensibly influences the eye in endeavouring to do the unfamiliar work in the heavens.

An Article has been published in the "Quarterly Journal of Science," No. xliv., for October, 1874, with title "On the Curved Appearance of Comets' Tails," by the author* of the Article on the Pole Star and Pointers in No. xliii. of that Journal.

The Author endeavours to explain that the "curved appearance" of the tail is commonly only an *appearance* due to the position of the tail in the sky, and *changing in curvature from hour to hour* in consequence of the change of apparent position due to the earth's rotation. The Author's argument is virtually simply a reiteration of the argument in his Article on the Pole Star and Pointers in No. xliii. of the "Quarterly Journal of Science." Anyone who has understood the explanation advanced in the present Article of the difficulty experienced by the eye in tracing Great Circles in the heavens will see that any *supposed change of curvature* of a comet's tail from hour to hour, due to its apparent change of position in the sky from hour to hour, is not—astronomically speaking — an "apparent change" of curvature, but is, in fact, for the most part, an OPTICAL ILLUSION.

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* Lieut.-Col. Drayson, R.A., F.R.A.S.

There is, however, an "apparent change" of curvature of the tail from hour to hour, due partly to *real changes of figure* in the tail itself, and partly to the *real change of position* of the whole comet in space. This "apparent change" of curvature is not noticed in the Article referred to.

It is the "apparent curvature," and "apparent change" of curvature of comets' tails due to their real curvature, and real change of curvature or real change of position in space, which are referred to in works on Astronomy. It seems incredible that all writers* on Astronomy up to the present time should have confused the cause of supposed visible curvature or change of curvature from hour to hour—which is here explained to be a mere OPTICAL ILLUSION—with the "apparent curvature" and "apparent change" of curvature (due to real curvature and to real change of curvature or real change of position in space).

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* An extract is given in the Article referred to from Herschel's "Outlines of Astronomy," Art. 557, as "showing some obscurity in the minds of former writers on astronomy on this subject of the curves of comets' tails." It seems needless to say, however, that Herschel there offers a physical explanation only of the *real curvature.*

PROGRESS IN SCIENCE.

MINING.

In a Memoir on the Geology of Darjiling, recently published by the Geological Survey of India, Mr. F. R. Mallet gives some interesting facts respecting the occurrence of coal in this locality. Although the late Dr. Archibald Campbell many years ago called attention to the reputed discovery of coal in this out-of-the-way corner of India, it was not until Dr. Hooker visited the country, in 1849, that true carbonaceous deposits were observed. The importance of a supply of coal for the great trunk railways of India has caused attention to be hitherto concentrated on the fields south of the Ganges; but the connection of Calcutta with the hill-country, by means of the Northern Bengal State Railway, has recently directed attention to the Darjiling coal, and has led to the investigation of the country by the Geological Survey. Mr. Mallet gives a list of localities where the coal-seams crop out. Most of these seams are highly inclined, and have been so violently disturbed that the coal is for the most part crushed, and even reduced to powder, so that it could be easily dug out, but would need to be compacted by artificial means in order to be available as fuel. The writer discusses the question whether it should be coked or prepared as artificial fuel, and inclines to the latter method.

Copper-mining has long been carried on in the neighbourhood of Darjiling, though only on a small scale and unsystematically. The ore is copper-pyrites,—not in true lodes, but disseminated through slates and schists. A description of the crude methods of mining and smelting the ores, as practised by the natives, is given by Mr. Mallet in his "Survey Memoir." He concludes that the prospect of copper-mining in this district is not inviting to European enterprise.

Some notes on the iron ores of Kumaon have been contributed to the records of the Geological Survey of India, by Mr. Theodore Hughes, who has also published some notes on the raw materials employed for iron-smelting in the Ranigang coal-field. The weak point in most of the Indian coals appears to be the large proportion of inorganic matter which they contain as compared with that of our own coals. A complete series of analyses of thirty different Indian coals has lately been made by Mr. Tween, under Dr. Oldham's direction.

In consequence of the reported discovery of a new coal-field in the Garoo Hills, a cursory examination of the district has been made by Mr. H. B. Medlicott, who thinks it safe to conclude, from his observations, that a coal-field of considerable extent exists in the very heart of these hills, the measures occupying an area of about 12 or 15 square miles.

Mr. V. Ball publishes, in the Records of the Indian Survey, a note of his visit to the coal-deposits recently discovered in the south-east corner of Afghanistan; and Mr. R. Bruce Foote describes his examination of the gold-deposits of the Dambal Hills, with the view of determining the sources whence the stream-gold of this region has been derived.

Large deposits of iron ore and extensive beds of coal occur in the neighbourhood of Wallerawang, about 100 miles from Sydney. These deposits have recently been visited and described by Prof. Liversidge, who has recorded his observations in an interesting paper. The ores—chiefly magnetic and brown iron ores—occur near the junction of the coal-measures with either Upper Silurian or Devonian rocks. The coal-measures contain several valuable beds, including three principal seams, of which the lowest has a thickness of 17 feet 6 inches, the middle seam 6 feet 6 inches, and the uppermost 4 feet 6 inches. Limestone occurs in the neighbourhood, and, on the whole, the district of Wallerawang appears destined to be one of the most flourishing mining centres in the Colony.

From Nova Scotia we have received the Annual Report of the Inspector of Mines, Mr. H. S. Poole. The statistics here given show that in 1874 there were in the Province 30 coal-mines, producing 872,720 tons of coal, and 33 gold-workings, yielding 9141 ounces of gold. Compared with the produce of the preceding year there is a falling off, which may in part be accounted for by the general dulness of trade. In addition to the statistical information, the Report contains some general observations, by Mr. Poole, on such branches of mining as are carried on in the Province.

The last number of the "Annales des Mines" opens with a long memoir on the Sulphur-Mines of Sicily, by M. C. Ledoux. During a visit to some of the more important sulphur-yielding localities, in 1871, he collected a good deal of information on this branch of industry; and although important memoirs on the subject have been written by Parodi and Mottura, the present paper presents—in a convenient and accessible form—a general sketch of the geological structure of the sulphur-producing districts, the working of the ore, and its treatment for the production of sulphur.

Mr. Warington W. Smyth, of the Royal School of Mines, recently delivered to the Iron and Steel Institute an interesting lecture on the Ores of Iron, considered in their Geological Relations. It is to be hoped that this lecture will be published in the Journal of the Institute.

METALLURGY.

At the recent metropolitan meeting of the Iron and Steel Institute the newly-elected president, Mr. Menelaus, devoted the greater part of his Address to a review of the chief modern improvements in the manufacture of wrought-iron and steel. The satisfactory working of Danks's system at the Erimus Iron-Works, near Stockton, was explained by Mr. J. A. Jones. The other forms of puddling machinery were noticed by Mr. Menelaus, who concludes that mechanical puddling is now an established success, and that better results may be thus obtained than by the old method of manual puddling.

Turning to steel, the president referred to the advantage of converting pig-iron into Bessemer steel by running the crude metal direct from the blast-furnace into the converter, and thus saving the cost of melting. With reference to the wide application of steel in the Arts, he gave the results of the experience of some of our leading mechanical engineers in favour of the use of a mild steel for purposes of construction.

The Bessemer medal has this year been awarded to Dr. C. W. Siemens.

In continuation of his valuable researches on the consumption of heat in the blast-furnace, Mr. I. L. Bell has recently read a paper "On the Sum of Heat Utilised in Smelting Cleveland Ironstone." He calculates that in the combustion of the best South Durham coke only 51·27 per cent of its heating power is utilised, the remainder being carried from the tunnel-head. In seeking to determine how this may best be utilised, he has been led to effect certain improvements in the construction of the boilers and hot-blast pipes at the Clarence Iron-Works, whereby the heating-power of the waste gases is so economised that all the heat needed for generating steam and heating air for furnaces is thus obtained, with some to spare, whilst in the old system, at the same Works, it was always found necessary to supplement the heat afforded by the waste gases.

An interesting process is now carried on by the Great Snowdon Copper Mining Company, for extracting copper from poor ores. The ore having been crushed is mixed with a small proportion of lime, and made up into a shape convenient for stacking in a kiln; the kiln of ore is then fired, and the roasted ore afterwards crushed and lixiviated. By this means the sulphide of copper originally present in the ore becomes converted into a soluble sulphate, and on passing sulphuretted hydrogen through the solution the metal is precipitated as a sulphide sufficiently pure to be at once smelted.

A paper on "The Extraction of Silver from Cupreous Iron-Pyrites," read before the Tyne Chemical Society, by Mr. T. Gibb, has recently been published in the "Chemical News" (April 16). After describing the ordinary process of treating burnt pyrites by the wet way, he refers to Claudet's method of extracting the small proportion of silver, and then describes his own process for effecting this extraction as practised by the Bede Metal and Chemical Company. This process consists in precipitating the greater proportion of the silver from the copper liquors by means of sulphuretted hydrogen, only a small proportion of copper being simultaneously thrown down. The sulphuretted hydrogen is economically obtained by the action of dilute hydrochloric acid on "tank waste." This sulphuretted hydrogen, mixed with carbonic acid, and diluted with a large volume of atmospheric air, is blown through the copper liquors for about twenty minutes. The precipitated sulphide of silver is calcined at a low temperature, and then ground and lixiviated, first with water, to dissolve sulphate of copper, and afterwards with a hot solution of common salt, to dissolve out the chloride of silver which is present. The solution is mixed with milk of lime, and the precipitate, having been washed, is digested in dilute sulphuric acid, and again washed and dried.

An improved method of preparing metallic barium has been devised by M. Sergius Kern, of St. Petersburg ("Chemical News," June 4). He precipitates iodide of barium by the action of iodine on barium hydrate, and then decomposes the iodide by heating it with metallic sodium. Barium is separated from the resulting mixture by means of mercury, and this metal is then distilled off from the amalgam, and the barium thus obtained in a free state.

MINERALOGY.

A short time ago a nickel ore from New Caledonia was described by Prof. Liversidge, of Sydney, under the name of *Noumeite*, after Noumea, the capital of the island. According to a paper since read before the Royal Society of New South Wales, it appears that the Rev. W. B. Clarke has suggested that the name of *Garnierite* should be bestowed on this mineral, since M. Garnier, a French geologist, some years ago obtained a similar mineral from New Caledonia. But the greater part of the nickel ore as shipped to Sydney consists of a similar mineral,—a hydrated silicate of nickel and magnesia,—but of a much darker apple-green colour than Garnierite, and to this second mineral Liversidge has now given the name of Noumeite, which he formerly applied to the paler variety. Both minerals are evidently products of decomposition, and Garnierite may perhaps be only a more highly altered form than Noumeite.

Prof. Church has contributed to the "Chemical News" (April 9) some "Short Notices of some Cornish Minerals." Specimens of a very pretty pink variety of soap-stone, resembling some of the Chinese figure-stones, were obtained from Mr. Talling, of Lostwithiel, and on analysis were found to consist of a hydrous silicate of magnesia, in which one-fourth of the magnesia is replaced by water: $3MgO.H_2O.4SiO_2$.

Some grains of gold from Ladock, in Cornwall, were found, by Prof. Church, to contain—Gold, 92·34; silver, 6·06; iron, a trace; silica and loss, 1·60. A fine specimen of filiform native silver, obtained many years ago from Huel Herland, in Cornwall, gave—Silver, 99·05; iron, 0·59. Neither gold nor copper was present, and the iron might be derived from traces of associated iron-pyrites.

Under the name of *Cossaite* Prof. Gastaldi has described a mineral apparently identical with paragonite, from which it differs in the absence of micaceous cleavage. It was found first in an antique ring dug up near Turin, but has also been found in other localities.

Several analyses of mineral phosphates extensively imported into Hamburg, from Estremadura, in Spain, have been contributed to a recent number of the "Chemical News," by Dr. B. C. Niederstadt. The average proportion of phosphoric anhydride is about 28 per cent.

An elaborate paper on the crystallisation of Galena, by Dr. A. Sadebeck, of Berlin, has been published in the "Zeitschrift" of the German Geological Society, where it is illustrated by three plates showing peculiar forms of this mineral.

Prof. Maskelyne, in a letter to Vom Rath, has announced the isomorphism of Asmanite, or orthorhombic silica, with Brookite, or oxide of titanium.

Some fine specimens of the American variety of Brookite (*Arkansite*) have recently reached this country; and we have also seen some splendid examples of plumbago, exhibiting in some cases a fibrous structure, brought over from Buckingham, in Canada, where it is now worked on a considerable scale.

ENGINEERING—CIVIL AND MECHANICAL.

Sanitation.—There are hardly two more important branches of engineering than sewerage and water supply, and these subjects have recently become prominent matters of interest by the introduction into Parliament of the Public Health Bill and the Rivers' Pollution Bill, two measures of the first importance, and largely called for in the interests of our town populations. It is not our intention to enter into any detailed examination here of the several clauses of these Bills, but we may do so upon another occasion. Everywhere the local authorities who have not already purified their sewage, are anxiously inquiring as to the best methods to be employed for that purpose; no scheme, however, yet invented has been found applicable under all the varying conditions that present themselves in different places, but amongst the numerous schemes that have already been put into practice, it may fairly be premised that one or other of them might be adopted—certainly with beneficial results, if not with absolute success—under all known circumstances. Where land is available, no doubt irrigation is an effective method of purifying the effluent water, but experience seems to teach us that in all cases some system of precipitation should be first adopted. The principal methods introduced for this purpose have been, on several occasions, described in the "Quarterly Journal of Science," so that it is not necessary to refer again to them now. A paper recently read by Mr. Charles E. Jones before the Chesterfield and Derbyshire Institute of Engineers, ably dealt with the subject of "Sewage; its Use and Abuse," and in the discussion that followed some particulars were given as to the value of excreta in Flanders, Belgium, and Bruges. In the latter place Mr. Chadwick stated that a system of employing servants existed whereby they were to receive a certain small money payment "*and the night soil*," and he further quoted statistics to show that the system of abolishing privies in England, and draining them into the sewers, has been followed by an enormous and alarming increase in the rate of mortality. The gist of Mr. Jones's paper was that the rainfall should go to the river, and the sewage to the soil, a distribution beyond doubt perfectly correct in theory, but hardly capable of being carried out except in comparatively small towns.

A paper on "Northampton and its Sewage" has also been recently read before the Association of Municipal and Sanitary Engineers, by Mr. J. H. Pidcock. Here the sewage was originally emptied into the river, but injunctions having been granted against polluting the river, notwithstanding the precipitation processes adopted there, it has been found necessary to purchase a sewage farm in order further to purify the effluent water before turning it into the Nene, thus bearing out what we have before remarked, that the soil must be depended upon to finish the purification of the sewage after the adoption of mechanical and chemical processes for extracting the solid matters contained in it.

In connection also with the general question of sanitation, we may also notice a paper read before the Institution of Civil Engineers on "The Systems of Constant and Intermittent Water Supply, and the Prevention of Waste," by Mr. G. F. Deacon. This paper had special reference to the restoration of constant service in Liverpool, and in it was explained the precautions taken by night inspectors to detect where a waste of water occurred, and, on the discovery of any such instance, measures are adopted to detect the cause and provide such remedy as might be necessary. The result of the trials thus far has been to show that with proper precautionary measures a constant service may be introduced so as to secure a lower consumption than under the intermittent system.

Ships.—The recent action taken in Parliament by Mr. Plimsoll has led to greater attention being given to the construction of shipping, and the extent to which this has taken place is shown in the nature of the papers read before the Institute of Naval Architects. Foremost amongst these is one by Mr. W. Froude on the "Rolling of Ships." This is a most important question as affecting the stability of vessels, but the difficulty in its proper determination consists in the theoretical nature of the necessary calculations, and the difficulty in ascertaining the actual resistance to oscillation presented under varying circumstances. In the absence of this resistance every ship of ordinary form would infallibly be overset the first time she encountered co-periodic waves. This is a very serious fact, and Mr. Froude has undoubtedly done good service in endeavouring to frame rules for the solution of so difficult a problem.

The superiority of steel over iron for shipbuilding has been ably explained by Mr. N. Barnaby, who showed that the French are in advance of this country in the adoption of the superior metal for that purpose. The principal care to be taken in its use is to avoid hammering the metal as much as possible, and to bend it to the necessary curves by means of pressure, so as to afford as little injury as possible during the process of construction. The chief advantage of steel is that it is homogeneous, and free from all laminations and blistering, and the recent improvements in its manufacture enable it to be turned out in sufficient quantities for the purpose in view.

An interesting paper by Mr. A. Sedgwick Woolley on "Spar Torpedo Warfare" was another contribution to the proceedings of the Institution of Naval Architects. In this paper the history of torpedo warfare is traced from the year 1775, and one Captain David Bushnell, of Connecticut, is said to have been the originator of the system, which, however, did not arrive at any really practical use until during the Civil War in America, when it was brought largely into use by the Southern States against the war vessels of their opponents. The results of experience gained up to the present time seems to be that the torpedo should explode only by actual contact with the vessel against which it is directed, because unless it is actually in contact with the side of a vessel, there is a great chance of the explosion failing to blow it in, since it will always take the line of least resistance. The plan of firing at will has the disadvantage of being left entirely to the discretion of the operator, who, in the darkness of night, under cover of which these attempts have to be made, may easily miscalculate the distance he is from a ship, and so fire his torpedo too soon. Some fast torpedo launches have recently been constructed by Messrs. Thorneycroft and Co., from the Swedish, Norwegian, and Danish Governments.

Within the last quarter there has been launched the twin-screw ironclad *Alexandra*, for the British Navy, which is justly described as the finest and most powerful broadside ironclad in the world. The following are her principal dimensions:—Length between perpendiculars, 225 ft.; extreme breadth, 63 ft. 8 in.; depth in hold, 18 ft. 7½ in.; tonnage, 6049 tons; indicated horse-power, 8000; and speed, 14 knots. The water-line of the *Alexandra* is protected by a belt 12 inches in thickness amidships, which tapers off towards the ends. The *Alexandra* is a central battery ship, and needs no bow or stern batteries to give her end-on fire; she has two gun-decks with end-on fire from both, and the batteries are armoured with 8 in. and 6 in. armour. The upper battery of this vessel consists of two 25-ton and two 18-ton guns, whilst in the lower battery are eight 18-ton guns.

Another vessel of special interest, which has recently been added to our commercial fleet, is the *Bessemer* steamer, designed primarily for cross

Channel traffic, and having a suspended saloon made unsusceptible to transverse oscillation by the application to it of special hydraulic machinery. This vessel is 350 ft. in length, and 40 ft. actual beam; she also possesses the peculiarity of having two distinct sets of engines, and two pairs of paddles, placed 105 ft. apart between centres; whilst, with the view of reducing the pitching motion as much as possible, she has been constructed with low ends. How far the vessel may prove a success is at present uncertain, and probably some further experience may be necessary before the swinging saloon is brought to such a state of perfection as fully to accomplish the desired end of insuring travellers against the terrors of sea-sickness.

Docks, &c.—A new public graving-dock has recently been opened at Cardiff in connection with the Roath Basin. The side walls of the dock are of Forest and Radyr stone, and the bottom of the whole is limestone, surrounded by a stone coping. Slips are provided each side, and the entrance, 60 ft. in width, is fitted with an immense caisson. The dock is 600 ft. in length, and 78 ft. wide; the depth of water provided will be, during spring tides, 23 ft., and during neap tides 13 ft., but the depth of water will rarely fall below 17 ft. The water can be discharged in about four hours, through the instrumentality of two powerful steam pumps.

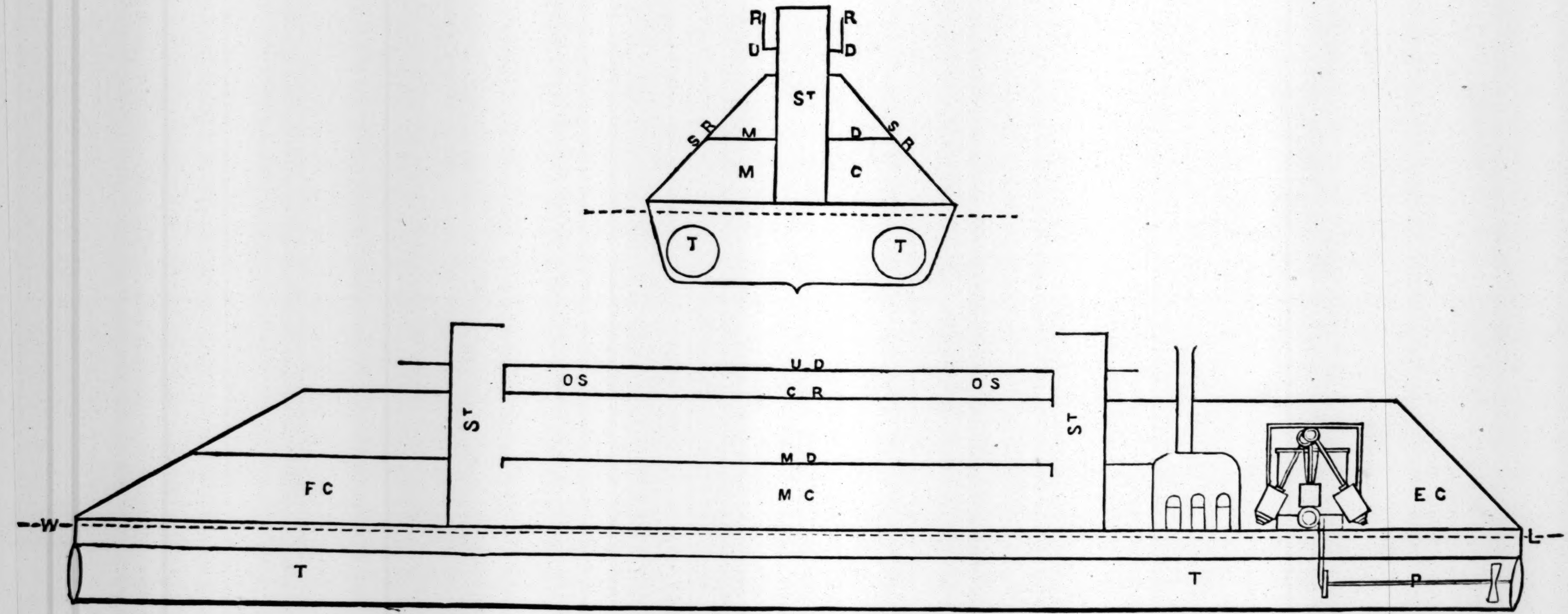
Railways.—A very great advance towards removing that element of human fallibility which has so often been the cause of serious railway accidents, has recently been made by the introduction of an electric block signal, the combined invention of Messrs. Farmer and Tyer. This apparatus comprises a combined mechanical and electrical arrangement by means of which the working of the semaphore signal can be controlled from any point, quite irrespective of distance. Electricity is employed to act on an electro-magnet, causing it to attract an armature; when the attraction ceases, a detent in connection with the armature is released, and the signal is set free from the hand-lever by a mechanical arrangement, and by means of the usual counterweight the semaphore on the line rises to and remains at danger directly the connection between it and the hand-lever is broken. By the use of this invention the signalman at either end of any block section has the power individually to act upon the semaphore to signal danger, but it is only by the concurrent action of the two that the safety signal can be given.

A very simple contrivance has recently been introduced upon the Great Western Railway of Canada, which promises to be of service by giving the engine-driver always a view over the whole train, and which promises to be of valuable assistance in the operating of goods trains, and would also be a useful adjunct for passenger train service. The device consists of the introduction of a mirror placed on the engine weather plate, immediately in front, and over the heads of the driver and fireman, at such an angle as to reflect a view of the whole of the wagons or carriages attached to the engine, and thus render the train behind as distinctly visible to the driver and fireman as the line itself in front.

At the time we are writing some important experiments are being made on the Midland Railway with different systems of continuous brakes, an account of which we hope to be able to give in the next number of the "Quarterly Journal of Science."

In San Francisco a system of wire rope street tramways has recently been introduced, which presents some features of novelty. In consists of an endless wire rope placed in a tube below the surface of the ground, between the tracks of the line, and kept in position by means of sheaves, upon and beneath which the rope is kept in constant motion during the hours the traffic is running, by a stationary engine, the power being transmitted from the motor to the rope by means of grip pulleys, and from the rope to the cars on the street by means of a gripping attachment fastened to the car, and which passes through a narrow slot in the upper side of the tube.

Dr. W. S. Carmichael thus describes his Breakwater Steamer:—The invention is intended to modify the influence of the waves, &c., by covering certain generally exposed parts of vessels, and thereby to lessen the dangers and inconveniences of the sea. It consists of one general principle, viz., the defence of certain generally exposed parts of vessels by a covering. 1st. The defence of the sides and decks by sloping roofs, thereby enabling the vessel to retain a more horizontal position, and abating the dangers and inconveniences of the sea. 2nd. The defence of the screw or screws by a covering, thereby saving it or them from shot, &c., and enabling it or them to act with more effect. It consists of a hull up to the water-level, W L, somewhat like that of other vessels, except that the keel is straight and horizontal, the bow perpendicular, and it is perforated by one or more openings to be afterwards described. I propose that the vessel shall be nearly flat-bottomed, the sides below the water-level nearly perpendicular, and also that the heaviest parts shall be on the outside. Near the water-line, W L, or several feet above it, according to the size of the vessel, the sides all round slope inwards, become a sloping roof (S R) over the cabins (M C, F C, E C) and main deck (M D), &c., at an angle of 45°, more or less, forming a breakwater on which the waves in rough weather may harmlessly expend their force, instead of dashing against nearly perpendicular sides, or washing the deck. These sloping roofs at the front of the vessel meet and form a sharp ridge, by which a wave at the front is cut in two. The upper part of this sloping roof, S R, may be made mostly of sliding panels, which may be pushed aside in calm weather, so as to render the main deck, M D, at such times nearly as open as in other vessels. Of course in the harbour these panels will always be drawn aside, so as to leave room for the entrance and exit of passengers, the carriage of luggage and cargo, &c. Seven feet, more or less, above the main deck, M D, the sloping roofs end in a flat roof, G R, to allow the more violent waves to pass harmlessly from windward to leeward, and, by being partly glazed, to give light to the decks below. The cabins below may also be lighted by the usual bull's-eyes. Three or four feet, more or less, above the partly glazed roof, there is a smaller upper deck, U D, about half or a third of the size of the main deck, for captain, pilot, &c., and from which the vessel may be steered. This is supported on iron rods, braces, &c., not represented in the illustrations, and also by having a round enclosed staircase (St) at each end, leading down to the decks and cabins below. I think this upper deck should be surrounded merely by a light railing, R, so as not to catch the wind. The moving-power is generally one or two propellers, P, within or immediately behind one or two tubes, T T, 2 to 8 feet in diameter, more or less, extending from stem to stern. If, on account of friction or other cause, these long tubes be found inexpedient, I propose two shorter tubes near the stern, either passing through part of the vessel, or altogether on the outside, or, finally, one tube from the stern with a branch passing through each side. By such means I expect that in calm weather such a vessel will advance through the water pretty much like other vessels—perhaps a little faster; that in rough weather the waves will rush harmlessly up the sloping roof and through the open space, O S, to leeward, and that the windward side of the vessel will not be so inconveniently raised, both on account of the heaviest parts being on the outside and on account of the shock of the waves being received on a sloping roof, which will cause that part to be depressed rather than raised; and my object is so to balance these forces, by varying the angle of the slope if necessary, as to keep the vessel steady. I expect, also, that the bow, by modifying the slope if necessary, will advance straight through a wave, instead of rising over it, and thus that the vessel will remain much more steady than is usual, and, as there will be less pitching, rolling, &c., that foundering and sea-sickness, and other dangers and inconveniences will be less likely to occur, and as its course will be shorter, passing through instead of over the waves, that it will also be more rapid. An open space for the waves to pass through, and a high deck beyond their reach, is an old idea of mine revived by the late discussions about the means for preventing sea-sickness. I expect, also, that as the water reaches the screw more directly, this will have more power; that the loss of power from slipping will



THE BREAKWATER STEAMER.

not be so great; and that the screw will act more as if it were rotating in an unyielding substance, or pumping the water from stem to stern. The screw will also be less exposed to danger from ropes, chains, torpedoes, shot, &c. When the wind is favourable, water sails may be used, or other sails with jury masts. I think that flat-bottomed boats built on this plan might be useful for landing passengers through a surf, as at Madras, and a modification of it for life-boats. In these cases a screw driven by a small steam-engine or strong arms would be necessary. Such a plan might enable fishermen to bring themselves and cargo home in safety instead of losing both by being swamped in a sudden storm. Such a plan in sailing vessels might save sailors and deck cargo from being washed overboard, as sometimes happens, and the vessel itself from foundering. In this case a high deck would be necessary, either on the top of the sloping roof, or made by connecting the fore, main, and mizen tops, or by both plans. When I explained my invention to a friend of mine here, he said —"Would you have your vessel lie on the surface of the water like a log of wood?" He expressed my intention almost exactly. I would have my vessel, in rough weather, lie on the water, or rather advance through it, like a log of wood sharpened at the front, the water washing every exposed part of it except the high upper deck. Taking this view of it, there seems to me some similarity between it and the catamaran of Madras: this is merely three logs lashed together; upon it one, two, or three men kneel, and paddle through the surf when no other craft can venture. In this case the catamaran and the legs of the natives washed by the waves may represent the body of my vessel and its roofs; the heads, bodies, and arms of the natives may represent my upper deck and its occupants. Another objection is that a violent storm would soon wash away all my sloping roof. This is easily answered: the first 6, 8, or 10 feet above the water-line must be made as strong as the bulwarks, or rather the sides of other vessels. Beyond 8 or 10 feet the force of the waves is not against the sloping roof, except the mere weight of the water, but obliquely upwards at the angle of the slope; and I would have the movable panels only about 6, 8, or 10 feet above the water-level. Another objection is that straight lines are wrong, because seamen liked a lively boat. My object is to prevent liveliness. Another is, that there would be a choking action in the tubes. This seems to me a question of power: increase the power and strength of materials, or diminish the size of the screw. The engraving is meant to represent a transverse vertical section through the after staircase, St, and a longitudinal vertical centric section, both about $\frac{1}{300}$th of real size, 1 inch representing about 25 feet. T T, tube or tubes for propeller or propellers; W L, water-level; M C, F C, E C, main, fore, and engineers' cabins; M D, main deck; S R, S R, sloping roofs, the bow and stern slooping roofs not lettered; G R, flat roof, partly glazed; O S, O S, open space for waves to pass through below upper deck; U D, upper deck; R R, railings round upper deck.

GEOLOGY.

Physical Geology.—Professor Prestwich, in discussing the origin of the Chesil Bank, has concluded that the shingle was originally derived from the cliffs between Budleigh Salterton and Lyme Regis, and that it was propelled eastward by the action of wind-waves, due to the prevalent and heaviest seas. Referring to the raised beach of Portland, he pointed out that therein were to be found all the materials noticed in the Chesil Bank. Remnants of this beach could be traced in or on the present cliffs, at intervals from Brighton to the coast of Cornwall, and it was to the destruction of this old beach that the pebbles in the Chesil Bank were largely due.

The rocks of the mining districts of Cornwall have been lately described by Mr. J. A. Phillips. He observes that the clay-slates of Cornwall differ materially in composition, but no rearrangement of their constituents could result in the production of granite.

Speaking of the origin of continents, Prof. Seeley stated (at a recent meeting of the Geological Society) that remembering that the lifting power of the moon coresponded to about 1-250,000th part of the earth's weight, and that the sun supplemented this with a power half as great, he ventured to suggest, since the earth's surface is thrown into folds which are proved, by fringing reefs and atolls, to be alternately rising and falling, that these movements might be explained, in part at least, as the effect of tidal movement in the earth itself.

Stratigraphical Geology.—The line of demarcation between the Cambrian and Silurian rocks has ever been a source of dispute amongst geologists, according as they held to the views of Sedgwick, or adopted the classification of Murchison. When first the term Cambrian was suggested by Sedgwick, and the term Silurian by Murchison, these two observers had been working at different areas and independently, and subsequent researches demonstrated that some of Sedgwick's higher Cambrian rocks were equivalent to Murchison's lower Silurian rocks. The former first demonstrated that the only great break in the entire series occurs at the base of the May Hill Sandstone, or Upper Llandovery group, the lowermost bed of Murchison's Upper Silurian. Murchison's classification has, however, prevailed; it has been accepted by the majority of geologists, until the last year or two, when a tendency has sprung up to adopt the classification of Sedgwick, because the labours of geologists in Wales and in the Lake District go to prove its truth. It may be observed that no base-line has existed for the Silurian system, that it has been constantly shifted to agree with the results of palæontological research, until nearly the whole of the old Cambrian system has been swallowed up in it by some writers. Professor Hughes recently observed, at a meeting of the Geological Society, that the bottom of the May Hill Sandstone constitutes the base of the Silurian system, and that the beds below, called Cambrian by Sedgwick, form a well-defined natural group, which, following the true principle of classification and justice to our nomenclature, we must adopt.

The development of the Kimmeridge Clay in England, and its palæontogical contents, have formed the subject of an important paper by the Rev. J. F. Blake. He would divide the formation into two sections, attaining together in places a thickness of 650 feet. Professor Seely described a new Chelonian obtained from the Kimmeridge Clay, which he termed *Pelobatochelys Blakei.*

Mr. Jukes-Browne has brought forward reasons for concluding that the Cambridge nodule-bed belongs to the base of the true Chalk Marl. This opinion is borne out by Mr. Whitaker from experience gained in mapping the Geology of the Cambridge district.

The Geology of the Burnley Coalfield and of the country around Clitheroe, Blackburn, Preston, Chorley, Haslingden, and Todmorden, has been described in great detail in a memoir published by the Geological Survey, the work of Professor Hull and Messrs. Dakyns, Tiddeman, Ward, Gunn, and De Rance. The work contains a valuable list of the fossils by Mr. Etheridge, and a Catalogue of all the works, numbering 561, that have been published to illustrate the Geology of Lancashire.

Palæontology.—Fossils indicative of a transitional zone between the Permian and Carboniferous strata were obtained at Spitzbergen by the late German Expedition.

Mr. James Thomson has described some forms of corals from the carboniferous limestone of Scotland which he regards as new species and as belonging to three new genera allied to *Clisiophyllum*, which he names *Rhodophyllum*, *Aspidiophyllum*, and *Kurnatiophyllum*. The specimens are particularly interesting from the evidences of variation which they present.

Dr. Dawson has noticed the occurrence of *Eozoon canadense* in Lower Laurentian rocks at Côte St. Pierre, on the Ottawa River. He referred particularly to the resemblance of weathered masses of *Eozoon* to Stromatoporoid corals.

An interesting section of the Rhætic beds is being opened up on a line of railway in process of construction between Melton Mowbray and Nottingham.

Prof. Huxley, in describing some new remains of *Stagonolepis*, observes that in outward form it resembled the existing Caimans of intertropical America, except that it possessed a long, narrow skull like that of a Gavial.

Geologists' Association.—At the Annual Meeting of this Association, held on

February 5th, 1875, Mr. W. Carruthers, F.R.S., was elected President; Mr. Etheridge, Professor Rupert Jones, Mr. J. L. Lobley, and Mr. Henry Woodward being elected Vice-Presidents.

Sub-Wealden Exploration.—It is to be regretted that the bore-hole, which had been carried to a depth of over 1000 feet, has been abandoned, owing to obstructions caused by accident to the machinery and loss of the tools. Six ineffectual efforts were made to recover them. The Diamond Boring Company have made a favourable offer to commence again, and Mr. Willett, the energetic honorary secretary of the Committee directing the Exploration, has guaranteed £600, and appeals for funds to carry on the enterprise. So far as the old boring is concerned, the results obtained are of great interest, and in a scientific point of view they are by no means unsuccessful; but the immediate object being to prove the depth of the Palæozöic rocks in the Wealden area, it is to be hoped that the second boring may obtain the desired information, which, while of so much interest to geologists, is most likely to throw some light on the vexed question of the probability of productive coal-measures beneath the secondary rocks of the eastern and south-eastern counties. A new boring has actually been commenced near to the old one, and a depth of 40 feet has been reached.

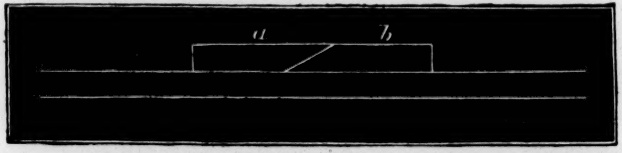
Geological Society of London.—At the Annual General Meeting of this Society, held on February 19, Mr. John Evans, F.R.S., President, in the Chair, the Wollaston Gold Medal was presented to Professor de Koninck, in recognition of his extensive palæontological researches, especially among the Carboniferous rocks. The balance of the proceeds of the Wollaston Donation Fund was awarded to Mr. L. C. Miall, to assist him in his researches on Fossil Reptilia. The Murchison Medal was presented to Mr. W Jory Henwood, F.R.S., &c., as a mark of the Society's appreciation of his observations on metalliferous deposits; and the Murchison Geological Fund was presented to Professor H. G. Seeley, in recognition of his various palæontological researches. In his address, Mr. Evans discussed the geological evidence of the antiquity of the human race.

PHYSICS.

LIGHT.—Writing to the "Chemical News," Mr. W. H. Olley draws attention to an adaptation which he made, about two years since, of polarised light to the well-known and beautiful optical instrument, the kaleidoscope. This instrument, it need hardly be observed, consists of a tube, within which are fixed, at an angle representing an aliquot part of a circle, two or three plane mirrors, blackened on the outside, so as to give by repeated reflection symmetrical and constantly varying images of pieces of coloured glass or other transparent objects, placed in a cell at one end of the tube, the images being viewed through a small aperture at the opposite end. It occurred to Mr. Olley that if pieces of selenite were substituted for the fragments of coloured glass, and polarised light were made to pass through the instrument, there would be seen, with the aid of an analyser, the beautiful colours given by that familiar double-refracting substance, reflected like those of ordinary light. Accordingly he selected films of selenite, presenting, when analysed, various colours corresponding with their respective thickness, and mounted them on pieces of thin crown glass of different shapes and sizes, placed them in the cell of a kaleidoscope, and adapted a tube to the other end so as to receive a second tube fitted with a small Nicol prism, or a tourmaline. He then found that when light polarised by a plate of glass, or even by a cloudless sky, was transmitted through the cell, the reflected images of the selenite mountings were symmetrically and beautifully displayed. To enlarge these polarised images, and to increase the strength of their colours by concentrating the rays, he adapted in some of the instruments a lens of appropriate focus to the eye-end of the tube just below the Nicol, or analyser, but this addition is not necessary.

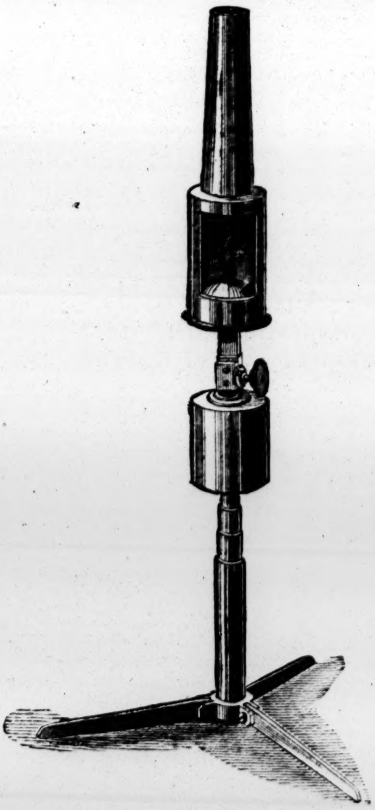
Microscopy.—Count F. Castracane ("Der Naturforscher") has demonstrated the existence of Diatomaceæ during the coal period. His first object of investigation was a piece of Lancashire coal, which was pulverised, and then exposed to a white heat. The decarbonised dust was then treated with nitric acid and chlorate of potash in test-tubes, and washed clean with distilled water, and then placed under the microscope. The Diatomaceæ found in this coal belong, with the exception of a *Grammatophora*, of a small *Coscinodiscus*, and of an *Amphipleura*, entirely to fresh-water genera and species, such as the following:—*Fragillaria Harrisonii*, Sm.; *Epithemia gibba*, Ehbg.; *Nitzschia curvula*, Kz.; *Cymbella scolica*, Sm.; *Synedra vitrea*, Kz.; *Diatoma vulgare*, Bong. The presence of the marine forms which were present among the very numerous fresh-water diatoms, only in one single specimen, appears to prove that, at one time, even sea-water found its way among the vegetation from which the coal originated. Besides this Lancashire coal, Count Castracane investigated coal of the carboniferous era from other localities, *e.g.* a piece of the so-called Cannel coal from Scotland, from Newcastle, and from the mines of St. Etienne. In every one of the pieces the presence of Diatomaceæ in greater or less numbers was proved. The species varied in the three different specimens of coal, but, as in the case of the Lancashire coal, not even a single new form was discovered, but all closely agreed with the existing fresh-water Diatomaceæ, from which they could not be distinguished by the most practised eye; all the signs by which the species of Diatomaceæ are generally distinguished are, in the Diatomaceæ of the coal period, identical with those of existing species; so that these organisms, in the indeterminably long period from the coal epoch to the present time, have undergone no perceptible modification.

Mr. Wenham communicates to the Royal Microscopical Society (March 3, 1875) an easy method of obtaining oblique vision of surface structure under the highest powers of the microscope. As the closeness of the high powers to the covering-glass will not permit the slides to be tilted to an extent to cause any appreciable difference in the appearance of an object, the following arrangement is to be adopted:—*a* is a slip of glass about $\frac{4}{10}$ths of an inch wide, ground and polished off to an angle. Objects to be mounted, such as diatoms or lepidopterous scales, are scraped up with the knife edge, so as to be distributed thereon along the sloping plane. Those situated near the edge may be viewed with the highest powers, as the glass is of course thinner here than any cover. The thickness of the remainder of the prismatic slip is of no consequence, and it may be of the same gauge as an ordinary thin slide. The slip is tacked on to a 3 × 1 slide with a dot of balsam or cement. Another similar slip *b* is then pressed endwise against it, so as to lay the objects flat between the two inclines. The lower prism is necessary, for without it a deal of offensive colour enters the object-glass from the decomposition of the transmitted light. This is recomposed or neutralised by the under prism, which also greatly increases the obliquity of the illuminating ray by refracting this to the same angle as that of vision, from the deflection of the axial ray of the object-glass. The degree of inclination of the facets of the prisms should be less than 40°, for on holding before a flame a slide having this angle, and tilting it slightly, the width of the junction of the prisms appears as a dark band impervious to light—the effect of total reflection. About 35° is therefore more suitable for objects mounted dry. If balsam is run in between the inclines, of course total reflection is eliminated and refraction nearly so, and we then see the object at an angle *the same as the incline*; therefore for objects in balsam 45° would be preferable. In using slides with

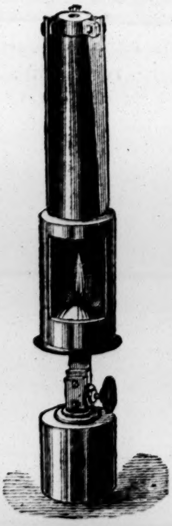


these prismatic or inclined mounts, it must be borne in mind that nearly every object lies under a different thickness of glass, according to the distance from the keen edge; therefore having selected a suitable one, the objective is to be carefully adjusted to give proper definition, and a thickness of glass over the scale may probably be found that will best suit its correction. For dry mountings, the object-glass must be used dry, as water would run in and spoil the object. If this is in balsam, of course the immersion system can be employed. The prismatic mounting slips may easily be made by any ordinary glass grinder. Thin well-polished sheet glass (such as is used for microscope slides) is cut into pieces, three quarters of an inch long by four-tenths wide. Eight or ten of these are cemented together with hard Canada balsam, and their step-like projecting edges adjusted against a bevel set at the desired angle, say 35°. They must be pressed into very close contact, as it is important to have the edges worked fine and clean, and any thickness of balsam stratum between them will prevent this, for during the work close edges mutually protect each other. Having prepared a sufficient number of blocks in this way, they are cemented on to a runner or metal plate with the usual cement of pitch and wood ashes. They are then ground and smoothed on a flat metal lap, till all the steps are gone and keen edges shown, and are then finally polished.

A lamp for the convenience of microscopists who are in the habit of using their instrument away from home has recently been contrived by Mr. Swift. It possesses the great merit of extreme portability in combination with the rare merit of perfect efficiency. The dimensions of the cylindrical case are 8¼ in. by 1½ in. The arrangements for packing the lamp are very simple, so that no time is lost in setting it up and putting it away. The burner is flat, of small dimensions, but the flame is quite large enough, and the light of good quality. The chimney is of metal, the lower part being cut away so as to



As in Use.



Packed Ready for Case.

allow a sufficient aperture for illuminating the microscope. The white lining is covered with glass, so that the smoke of the lamp cannot blacken it. Small as the reservoir appears to be, it contains oil enough to last four hours. The means taken to prevent leakage are very ingenious; a carefully-fitted screw-cap prevents leakage at the burner, but the greatest ingenuity is displayed in fitting the pinion, which raises the wick to prevent escape of oil. So effectual are the precautions taken to keep in the oil, that the writer received the lamp by carrier, and has since carried it inverted in his pocket, without the slightest inconvenience from leakage. The flame can be adjusted at any level from about 4 to 12 in. The general form of the lamp can be easily understood from the accompanying woodcuts.

HEAT.—Under the supervision of Dr. R. Carter Moffat, a large party of gentlemen connected with shipping and the Board of Trade recently assembled at Greenhithe, near Gravesend, to witness the power of Paton and Harris's Pyroletor to extinguish fire in closed places. The pyroleter consists of a small double pump worked by hand, which sucks up from tubes on either side of it strong muriatic acid and a solution of bicarbonate of soda, which commingle in a generator forming part of the pump, and the carbonic acid gas and solution of salt pass at once down a metal pipe to the hold, along whose keelson runs a perforated wooden box which admits of the gas passing through to the burning material. The agent, therefore, for the extinction of fire is dry carbonic acid gas, which has no action on cargo. A steamer conveyed the party from Blackwall to Greenhithe, where a large wooden barge had been prepared for the experiments. Its entire hold was covered to a depth of several feet with wood shavings, cotton-waste saturated with turpentine, and naphtha. A temporarily-raised and by no means air-tight wooden deck, with loosely-fitting boards, formed the wide hatchway covering. After the apparatus had been explained by Dr. Moffat, the pipes to the chemicals were attached, and the signal given to set fire to the inflammable materials in the hold. Immediately the flames ran along the entire cargo and issued above the temporary deck, which was then covered with boarding. The pyroletor having been brought into action, and although nearly half a gale of wind was blowing, the fire was completely extinguished in four minutes. The experiments were so completely successful, and the efficiency of the apparatus so apparent, that the party at once agreed to sign a memorial to ask Government to compel all long-passage ships conveying passengers and cargo to carry one of these instruments. It is computed that a 1200-ton ship requires about half a ton of each of the chemicals, which, with their packages, cost about £20.

ELECTRICITY.—From a report by Count du Moncel on the thermo-electric battery of M. Clamond we gather that, in this battery the electro-positive element is of iron, and the negative element an alloy of antimony and zinc: and, as in the arrangement of Mr. Farmer, the elements are connected circularly for intensity, forming a kind of crowns, isolated from each other by plates of amianthus, and having their polar extremities placed in connection with a commutator, fixed tangentially to the cylindrical surface of the apparatus, and contrived so as to cause these crowns to be grouped either for intensity or for quantity, as may be requisite. The apparatus has been employed for six months at the galvano-plastic works of M. Goupil at Asnières. The gas consumed amounts to 2½ frs. per kilo. of copper deposited.

M. J. Morin has invented a new electro-medical galvanoscope. According to the description in the *Comptes Rendus* it consists of an ordinary two-branch electro-magnet, placed vertically, the breach being in the air. A magnetic needle is suspended by one of its poles over the breach, through which it penetrates by means of a large hole. The lower free pole of the needle descends as far as the level of the lower part of the electro-magnet's helices, between which it is able to oscillate. The needle is long enough to penetrate the breach to the height of its neutral point, thus nullifying at that spot all reciprocal action. On making a current circulate in the helices the two poles act in the same direction upon the free pole of the magnetic needle, causing it to be displaced towards one of the helices according to the direction of the current.

TECHNOLOGY.—A Board has been appointed by the Government of the United States to determine by actual tests the strength and value of iron, steel, and other metals which may be submitted to it, or by it procured, and to prepare tables which will exhibit the strength and value of said materials for constructive purposes. It has standing committees on abrasion and wear, on armour plate, on chemical research, on chains and wire ropes, on corrosion of metals, on the effects of temperature, on girders and columns, on metallic alloys, on physical phenomena, on steels produced by modern processes, &c.

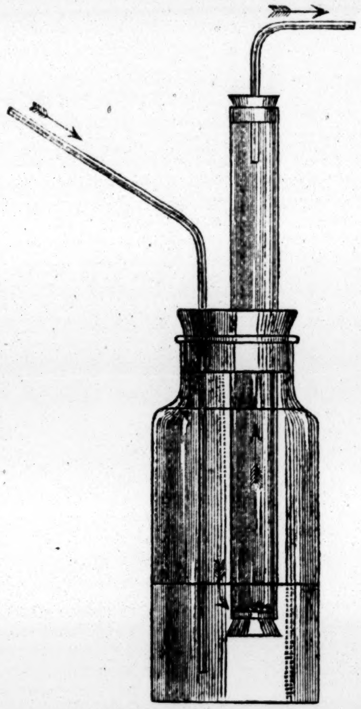
Th. Schlösing, in a course of studies on arable soils, has examined the influence of the salts present in a soil upon its openness of texture. Mould stirred up in water subsides the more rapidly the more salts are present, especially those of lime and magnesia. This phenomenon, which amounts to a coagulation of the clay, is thoroughly examined. The best precipitants for clay are caustic lime and the lime salts.

A good cement for marble and alabaster may be obtained by mixing 12 parts of Portland cement, 6 parts of slaked lime, 6 parts of fine sand, and 1 part of infusorial earth, and making up into a thick paste with silicate of soda. The object to be cemented does not require to be heated. It sets in twenty-four hours, and the fracture cannot be readily found.

M. C. H. Viedt has experimented on aniline inks. For a red ink he recommends a solution of 1 part of "diamond fuchsin" (we should say of Brooke, Simpson, and Spiller's rosein) in 150 to 200 parts of boiling water. For a blue, he dissolves 1 part of bleu de Paris in 200 to 250 of water. For a violet, 1 part of a Hofmann's violet (blue shade) in 300 of water. A beautiful green ink is made by dissolving 1 part of iodine green in 100 to 110 parts of boiling water. The yellow aniline inks are not recommended. These inks are not fit for copying, but they have the advantages of drying quickly, and of never clogging.

Prof. J. Nessler points out that genuine wines contain chiefly malic acid. Free tartaric acid is very rarely found, except in spurious concoctions. As a test the author uses a solution of 5 grms. acetate of potash, 5 grms. alcohol, and 25 grms. water. If an appreciable amount of tartaric acid is present, this test produces a crystalline deposit of tartar in a quarter of an hour; whilst, in genuine wines, even if they contain a trace of tartaric acid, no precipitate appears until some hours have elapsed. Genuine wine contains no citric acid. For its detection in small quantities, the wine is rendered alkaline and filtered, acidulated with acetic acid, mixed with chloride of barium, filtered, and a few drops of ammonia added to the filtrate until it has an alkaline reaction. If, on the addition of baryta water, a white precipitate appears, citric acid is present. Oxalic acid gives a white precipitate if lime-water is added in such small quantities that the liquid has still an alkaline reaction. Sulphuric acid in genuine wines is found only to the extent of 0·03 to 0·05 per cent.

When soluble gases are highly diluted with those which are insoluble, their complete absorption is a matter of difficulty when attempted by drawing the mixture through simple columns of the absorbent liquid, owing to the protecting effect of the particles of insoluble gas. In such cases, and with the ordinary wash-bottle arrangement, complete absorption can only be effected by greatly multiplying the pieces of apparatus. Mr. N. Glendinning has arranged a new form of apparatus, of which the annexed is a drawing. It consists of a bottle capable of holding 20 fluid ounces, which is provided with two tubes passing through a perforated india-rubber bung. The tube which has to convey the gases to the apparatus has a bore of about $\frac{3}{16}$ths of an inch, and reaches to within half an inch of the bottom of the bottle. The other tube is about 12 in. long and 1 in. in diameter. One end of this tube is closed, but is provided at about half an inch from this end with twelve or fifteen small holes distributed around it, forming a ring, the holes being equidistant from the end of the tube. The other end of this tube is fitted with a cork, through which passes a small tube. To prepare the apparatus for use, about 8 ounces of the absorbent liquid are put into the bottle, the larger tube partly filled with glass splinters, and depressed so that the apertures dip about half an inch into the liquid. The conveying tube is now introduced into the pipe or chamber from which the sample has to be taken, and the aspirator attached to the other. The liquid will now rise in the larger tube until that in the bottle has been drained to the level of the apertures, and the gases which have already been partly washed in their passage through the stratum of absorbent liquid will now pass through the small apertures, and through the column of liquid in the tube, where their absorption is favoured by the breaking-up action of the glass splinters, and the agitation consequent on the passage of gas bubbles. When a sufficient quantity of the gases has been drawn through the apparatus, it is detached and shaken, so that the soluble gases existing in the atmosphere of the bottle may be absorbed. The cork is now removed, the glass splinters thoroughly rinsed into the bottle, and the liquid made up to any required bulk.



A paper on "Toughened Glass" was recently read before the Society of Arts by Mr. Perry F. Nursey, C.E. The subject being of importance, we do not hesitate to give a somewhat lengthy notice of the paper. After a reference to the origin and progress of the manufacture of glass, Mr. Nursey says:—Many years since M. de la Bastie was impressed with the desirability of rendering glass less brittle, and so extending the sphere of its usefulness. Aware that the fragility of glass results from the weakness of the cohesion of its molecules, he argued that, by mechanically forcing the molecules closely together, and rendering the mass more compact, the strength and solidity of the material should be increased. This is exactly the line of argument an engineer would follow—it is one which led Sir Joseph Whitworth to produce such splendid results in the well-known Whitworth metal, and it is one also which has led to success in casting in other departments of engineering. It is, however, not one which landed M. de la Bastie on the right side of all his hopes and fears, inasmuch as he found, after long trial and experiment, that mechanical compression failed to influence glass in the slightest degree, even when applied while the material was in a fluid or soft condition. He therefore changed his tactics, and commenced to apply to glass a system of tempering, such as is usually applied to steel, namely, submitting it to a bath of heated oil. He knew well that by immersing heated glass in cold water he would only put the material in a state of unstable equilibrium, so that the least shock would cause it to break up, as in the case of the Rupert drops. He then sought to invert this result, to diminish, or even to remove, the extreme fragility of glass by tempering it by immersion in a fluid other than water. In attaining this object two essential objects had to be determined. First, the point at which glass can be tempered without being put out of shape, and secondly, the medium to be employed for the immersion of the glass. The first condition M. de la Bastie found to be that degree of heating at which softness or malleability commences, when the molecules are capable of closing suddenly together, condensing the material when it is plunged in a liquid at a somewhat lower temperature. The second condition he found was satisfied by having a fluid capable of being raised to a much higher temperature than that of boiling water, without entering into a state of ebullition. For this purpose, and after a long series of experiments, M. de la Bastie devised an oleaginous compound, formed of oils, wax, tallow, resin, and other similar ingredients in certain proportions. Although the invention is apparently a most simple one, there are many delicate conditions involved, the disregard of any one of which constitutes the precise difference between success and failure. It thus happened that, seven years since, just as M. de la Bastie had perfected his invention, and had produced highly satisfactory results, he lost the clue to his success, and for two years was baffled in every attempt to re-discover it. He at length succeeded in regaining his secret, and has since been engaged in perfecting his invention, and putting it into a practical shape. He had to carefully adjust all the numerous details, for although the invention consists in simply heating the glass, and dipping it while hot into a heated oleaginous bath, there are many conditions involved. Thus glass articles may be underheated, and will not be susceptible to the effect of the bath, or they may be overheated, and will lose their shape; or, again, they may be heated to the right temperature, and yet be spoiled during the process of transference into the bath. Then, again, the exact proportions of the oleaginous constituents of the bath, and their precise temperature, have an important influence upon the ultimate result. All these points, however, with many others, have been definitely settled by M. de la Bastie, who has for some time past worked his process experimentally, and is now erecting a factory in France, in which to carry it on practically and commercially. It may be as well that I should here mention that it is recorded by Pliny, that in the reign of Tiberius a combination was said to have been devised by which a flexible glass was produced, and that the machinery by which it was made was destroyed in order to prevent a depreciation in the value of the precious metals. We have, however, no evidence that this was the toughening process invented by M. de la Bastie, and the statement to which publicity has recently been given, in no whit detracts from the merits of that gentleman as the inventor of an important economic process. Nothing more than the bare fact above alluded to is on record, except it be, perhaps, that the hapless inventor was destroyed as well as his apparatus. But there was no Society for the Encouragement of Arts, Manufactures, and Commerce in those days. In carrying out his process, M. de la Bastie finds it necessary to raise the glass to be tempered to a very high temperature. The hotter it is the less the risk of breaking the glass, and the greater the shrinkage or condensation. Hence the advantage, and often the necessity, of heating the glass to the point of softening, which is attended by the difficulty that glass in the soft condition gets readily out of shape, so that it must be plunged into the bath almost without touching it. In plunging the hot glass into a heated combustible liquid the latter is apt to take fire, and cannot easily be extinguished, so that time and material are lost. These difficulties M. de la Bastie has overcome by placing the tempering bath in immediate communication with the heating oven, and covering it so as to prevent access of air. The oven being charged with the articles to be tempered, they are made to slide into the adjoining bath without being handled, and the contents of the bath, having no supply of external air, are not liable to inflame. In order that the shape of the tempered articles may not be affected, particularly flat glass, the floor of the oven is made to cant, so that, when the glass is heated on it, it is turned to a sloping position, and the glass slides into the bath, along a surface arranged in it at the same angle as that of the oven floor. The clearness of the glass may be affected by the dust of the furnace flame, which is apt to settle on the glass and chill its surface. This is avoided by heating the glass in a muffle, to which the flame has no excess, being applied externally. The shock of the fall of glass into the bath is prevented by fixing in it a sheet of wire gauze, or asbestos fabric, for the glass to fall on. Of course the condition of working would be considerably modified where glass manufacturers adopted the toughening process in their own works. In such case the toughening process would simply take the place of the present annealing process, than which it is much more speedy and economical. The glass would then be treated just at the point at which it passes from the fluid to the solid condition, and would not require reheating. By the substitution of this process for that of ordinary annealing, the saving would be considerable. There would be, first, the saving of the fuel used in the annealing ovens; next, the saving of the time required for annealing; thirdly, the saving in breakages, besides a saving in labour as well as in other directions. The physical change which glass thus treated undergoes is no less complete than remarkable. Its extreme brittleness is exchanged for a degree of toughness and elasticity, which enables delicate glass articles to be thrown indiscriminately about the room, and more substantial ones to resist the impact of heavy iron weights falling from considerable heights. Upon my first making the acquaintance of toughened glass articles at the offices of Messrs. Abel Rey and Brothers, 29, Mincing Lane—Messrs. Rey being the representatives of M. de la Bastie—watch-glasses, plates, dishes, and sheets of glass, both coloured and plain, were thrown across a large room, and fell spinning on the floor. Water was boiled in a tempered glass saucer for some time over a brisk fire, and the saucer was quickly removed to a comparatively cold place, and stood on iron, but was in no way affected by change of temperature. A small piece of plate-glass was held in a gas flame until the corner became very hot. The glass proved a bad conductor of the heat, which did not extend any appreciable distance beyond the point of contact with the flame, neither was the glass cracked from unequal expansion, nor was it damaged by sudden immersion in cold water. In order to judge of the comparative resistance offered by untoughened and toughened glass to the force of impact, a piece of the former, measuring 6 inches by 5 inches, by one-eighth inch thick, was supported in a frame about half-an-inch from the floor. A 2-ounce brass weight was then dropped upon it from 12 and 18 inches respectively without damage, but on the height being increased to 24 inches, the glass was broken into several fragments. A piece of toughened glass of the same size, but rather thinner, was then treated in the same way, at heights increasing a foot at a time up to 10 feet, but without producing the slightest visible impression. I say "visible" impression, because it is possible that, by the repetition of the blows, the structure of the glass may have become imperceptibly altered. We all know that by repeated blows the fibrous nature of wrought-iron becomes exchanged for the crystalline character of cast-iron. Finding the 2-ounce weight to make no impression, an 8-ounce iron weight was substituted, and was dropped on the glass from a height of 2 feet, and then of 4 feet, without fracturing it. On the height being increased to 6 feet, however, the glass broke with a distinct report. But here another phenomenon presented itself; instead of the toughened glass being broken into some twelve or fifteen large angular pieces, as was the ordinary glass, it was literally reduced to atoms. There were, it is true, some pieces about half-an-inch square, but these were traversed in all directions by delicate lines of fracture, and, on being gently touched, crumbled into small pieces; and many of these small pieces were easily reduced by gentle pressure into mere atoms, so thorough and so complete does the disorganisation of the entire mass appear to be. All these points will be practically demonstrated at the conclusion of my paper. A similar result is produced by placing a piece of toughened glass flat on the table, with a corner projecting over, and endeavouring to chip the corner off with a hammer. The corner will, after a series of smart blows, be broken off, but the whole mass will be at the same moment disintegrated and reduced to atoms. Another peculiarity about toughened glass is that the fragments are by no means so sharp, and therefore so capable of piercing the flesh, or of causing incised wounds, as are those of ordinary glass. One important point of difference between M. de la Bastie's toughened glass and Prince Rupert's drops is that, although the skin of the former may be scored through with the diamond, the body cannot even then be broken through by ordinary force, much less does the mass fly to pieces and disintegrate, as in the case of the Rupert drops. Still wider will this difference appear when I state that toughened glass is readily susceptible of a high degree of polish, and it can be cut by the wheel for lustre-work and such like. The glass can likewise be engraved, either by hydrofluoric acid, or by Mr. Tilghman's elegant sand-blast process. It will thus be seen that toughened glass presents features which appear to some extent paradoxical. It would appear that toughened glass possesses enormous cohesive power, but that if the equilibrium of the mass is disturbed at any one point, the disturbance, or disintegration, is instantaneously communicated throughout the whole piece, the atoms no longer retaining the power of cohesion. It is as though the glass was endued with a nervous system, a shock to which at any one point instantly and utterly demoralised the whole. It is important to note that neither transparency nor colour in glass is in any way affected by the process of toughening, and the ring, or sound emitted upon the glass being struck, is nearly as clear in toughened as in plain glass. In order to determine the relative values of ordinary glass and the toughened material, as regards their strength, I suggested to Messrs. Rey the propriety of instituting experiments, with the view of ascertaining their respective resistances to ordinarily applied stress. In these experiments I have been ably assisted by Mr. Kirkaldy, whose perfect testing machinery has, for some years past, supplied a want long previously felt by the engineering profession. Twenty pieces of glass in all were submitted to bending stress, ten being toughened and ten untoughened. The glass was of French manufacture, and was that known as "Rive de Giers." Each piece of glass measured, as nearly as possible, 6 inches in length by 5 inches in breadth, and the samples had a mean thickness of 0·2259 of an inch. Each piece was placed with a bearing of half-an-inch at each end, and the weight was brought gradually upon the centre; in some instances by the testing-machine, and in others by direct weights. Taking two pieces of glass, having about the same sectional area—the one tempered and the other untempered—Mr. Kirkaldy's certificate shows that the untempered glass yielded under a strain of 279 lbs., whilst the toughened glass did not give way until a stress of 1348 lbs. had been reached. The same proportion, however, did not occur throughout the series, the toughened glass giving in some instances lower results. This arose from two causes—the diminished area of some of the samples of glass, and from the fact that, in some instances, the process of toughening had not been perfectly carried out; for the samples were prepared by M. de la Bastie under purely experimental conditions. The imperfect tempering was made manifest, after the destruction of the glass, in three ways chiefly; first, by the glass showing needle fractures, such as are seen in untoughened glass; secondly, by a faint milky line presenting itself in looking at the glass in section; and, thirdly, by portions of the glass, a square inch in area, remaining unfractured, whilst the whole surrounding mass was reduced to atoms. But above and beyond all this, it was evident that the strains applied were such as could not possibly come upon glass articles in ordinary use. They were long-sustained pressures, tending at every increment of weight to alter the relative position of the particles of the glass, but affording them no opportunity of returning to their normal position, or, in other words, of utilising the elasticity of the mass.

Glass articles in ordinary use are subject to sudden sharp blows, either from falling down, or from some extraneous substance being brought smartly in contact with them. Under these conditions the elasticity of toughened glass is called into play, and enables it to sustain a shock immeasurably beyond that which would suffice to destroy ordinary glass, as is shown by the experiments first described. Hence the proper tests for glass, either toughened or plain, are precisely those of smart and sudden impact, and not of prolonged stress. Examination and experiment with this remarkable substance have revealed a number of most interesting facts with regard to its physical character. The limits of a paper, however, forbid me entering upon these considerations at such length as I could wish, and as the subject deserves. I may, however, mention that the microscope reveals the fact that the fractures follow a regular order, which gives a uniform shape to the crystals which they produce. Large crystals can be subdivided into several smaller ones with a similar result. The edges of the atoms, too, are not jagged and serrated, as are those of ordinary glass, hence their diminished tendency to cause incised wounds, as already mentioned. This peculiarity would afford a means of ascertaining whether the glass had been tempered or not. The physical character of toughened glass has been made the subject of careful investigation by M. Victor de Luynes, who made the results of his researches the subject of a lecture, which he delivered at the annual meeting of the Société de Secours des Amis des Sciences. I hoped to have embodied in my paper some of the results obtained by M. de Luynes, but, unfortunately, the rules of the Société do not permit the publication of lectures until they have passed the examination of a committee, which process M. de Luynes' paper is only now undergoing. I may, however, mention that M. de Luynes had a furnace and bath in the lecture-room, and before his audience he tempered glass objects, which were afterwards successfully tested. As a general result, M. de Luynes has found that toughened glass will bear from 80 to 100 times the strain of ordinary glass. M. de Luynes also examined both plain and toughened glass by the aid of polarised light, the results of his examination going to show that toughened glass owed its peculiar characteristics to a condition of intensified compression. I have explained what toughened glass is, how it is produced, and what are its leading features, so far as at present ascertained. It therefore only remains for me to indicate the direction of its practical application. I say "indicate," for were I to enumerate all the purposes to which it can be usefully applied I should simply become wearisome. It is possible that there is not one corner in the whole domain of the arts, sciences, and manufactures, where its presence will not in time be made manifest in some way or other, and its usefulness appreciated, whilst for purposes pertaining to social life its application would seem to be unlimited. The miner would have a safer safety lamp than even Davy gave him, and the engineer's gauge glass would stand the highest steam pressure and alternations of heat and cold without fear of mischance. In chemical works it would supersede lead for tanks, and the present costly and unreliable glass pump-tubes would be far less expensive, and infinitely more durable. So with brewers; they would find it a most useful friend in their vats, which they could thoroughly and easily cleanse, and keep free from those secreted stale germs of organic life which develop and reproduce themselves in the process of fermenting beer, in a highly objectionable manner. For water-pipes it would offer the advantages of strength, without corrosion. Assayers, I am told, would use it instead of platinum in some processes. In silk-spinning machinery, slider-eyes, or guides, which are so soon cut through by reason of the speed at which the silk passes through them, would be rendered very durable if made of toughened glass. Another application, which has suggested itself to an ingenious American gentleman since the first notice of M. de la Bastie's invention appeared, is the manufacture of printing-types, and rollers for printing-presses, and this idea is now being developed into practical form. Seeing the wide range of domestic and social wants which toughened glass promises to meet, I know not where to begin, and were I to begin I should not know where to end. I can only observe in this connection that toughened glass promises to supersede porcelain and similar wares, and to add a real and permanent value to glass utensils of every kind. It will probably supersede enamel on culinary utensils, and in other similar directions. It might be thought that this invention would prove a disadvantage to the glass trade, but the widely-increased use of glass for purposes which its brittleness has hitherto unfitted it should be a sufficient answer to any such objectors. If there were any such, they would naturally be those connected with the glass trade. But I do not find them there. On the contrary, some of our most eminent glass manufacturers are now negotiating with M. de la Bastie's agents for licenses to work the patent in England. I may observe, that it is not at all improbable that the invention will receive its first practical application in the Aquarium now in course of erection within a short walk of the house wherein we are now assembled. Such, then, is one of the most notable inventions of modern times, an invention so remarkable, so unique, and apparently so fraught with import to the arts, sciences, and manufactures, as to render it probable that the name of De la Bastie will one day occupy no mean position amongst those of men by whose genius science has been enriched, and the nations practically benefitted.

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